Taxes, informality and income shifting: Evidence from a recent Pakistani tax reform[☆]

Mazhar Waseem

Department of Economics, University of Manchester, Arthur Lewis Building, Oxford Road, Manchester, United Kingdom

ARTICLE INFO

ABSTRACT

This paper examines firm behavior to taxation in a low enforcement and large informality setting. Using quasi-experimental variation created by a tax reform, which increased taxation of partnerships substantially relative to firms of other legal form, and the population of income tax returns filed in Pakistan in 2006–11, I document that treated firms report significantly lower earnings, migrate into informality, and switch business form in response to the increase in tax rate. The revenue loss caused by these behavioral responses is so large that by the third year after the reform the government was collecting less revenue than it would have without the tax increase. This implies that the new tax rate was on the wrong side of the Laffer curve and would not have been optimal under any social preferences. The richness of the data and tax variation permits me to decompose the observed responses into real and evasion margins and to identify fiscal externalities created by the reform on other tax bases. The welfare cost of the reform increases by around 40% once these externalities are taken into account.

1. Introduction

The presence of large informal sector constrains taxation capacity of developing countries in two important ways.[1] First, there is a direct effect as taxation base is limited to a narrow set of formal taxpayers. Second and more subtle is the indirect effect: governments in these countries tend to keep tax rates low fearing that increased taxation might unravel the already thin formal sector.[2] Whether such fears are justified depends on the elasticity of the tax base, in particular on how likely the taxpayers are to exit into informality in response to a tax increase. There is quite a large literature that estimates the sensitivity of the tax base to the marginal tax rate using administrative tax return data (Saez et al., 2012), but unfortunately most of this literature is set in rich countries and the corresponding evidence for developing countries is limited. In fact, to my knowledge there is no micro-based study that takes into account the movements into and out of informality, which arguably is a more important margin of response to taxation in a developing country setting. This paper fills the gap by presenting evidence on the responsiveness of earnings, formality and business organization choices of agents to personal income taxation in Pakistan.

For this purpose, I exploit a natural policy experiment created by an income tax reform introduced in the country in 2009. Before the reform earnings of noncorporate firms – sole proprietorships and partnerships – were taxed lightly relative to earnings of corporations, and it was felt that the distortion was preventing the incorporation of new firms. The reform raised the income tax rate on partnership earnings to a flat 25%, thus neutralizing largely a partnership's incentive to stay unincorporated. As an unintended consequence, however, it created a large tax rate variation within noncorporate firms: partnerships experienced on average a greater than five-fold increase in tax rates from 2009, while rates applicable to sole proprietorships remained unchanged in 2009 but reduced slightly from 2010 when their tax schedule was revised.[3] These differential changes in tax rates over time and across very similar firms create an almost ideal experiment to study firm behavior to taxation in a low enforcement-capacity setting.

One other interesting feature of the reform is that it was given a retroactive effect. The tax increase was announced on June 6, 2010, but it was made applicable from the beginning of the tax year *i.e.* from July 1, 2009. Thus, by the time firms learnt the tax change 94% of the tax year 2009 had already elapsed. Generally, behavioral responses to taxation conflate real and evasion margins and there is no satisfactory way to separately identify the two. The retroactive applicability,

---

[1] According to a recent survey (Fuest and Riedel, 2009), informal sector on average constitutes one-third of the official economy in developing countries.

[2] The starting income tax rate, for example, in Pakistan during 2006–2009 was just 0.5%.

[3] Because the tax schedule is not indexed to inflation, such revision, involving the movement of bracket cutoffs, is needed every three to four years to avoid bracket creep.

however, allows me to disentangle tax evasion and real response in a transparent manner: while the post-2009 response to the reform could encompass both margins, the 2009 response would comprise tax evasion mainly. This interpretation rests on the assumption that the reform was not known before its official announcement. Tracking the entry of treated and untreated firms over time, I provide a comprehensive test confirming that the reform indeed was not anticipated.

I use administrative data from the Federal Board of Revenue, Pakistan (FBR), which comprise the population of income tax returns filed in 2006–11 and a set of firm characteristics. To guide the empirical analysis, I set up a simple model of firm behavior, characterizing the revenue and welfare implications of the tax change in terms of estimable behavioral elasticities. The empirical strategy, motivated by the differential changes in tax rates across firms and over time, compares the evolution of partnership outcomes with that of sole proprietorship and corporate outcomes in event-study research designs. The claim here is not that a firm's organizational form is randomly assigned; it is rather that the outcomes would have evolved similarly had the tax rates not changed.

In the initial set of empirical results, I provide nonparametric evidence cataloging four important impacts produced by the reform. First, following the tax increase the number of formal partnerships declined dramatically: by 41% in 2009, by another 27% in 2010, and by an additional 15% in 2011. This means that within three years of the tax increase, the number of partnerships in Pakistan had declined to 36% of the baseline level. Second, partnerships which did not exit reported considerably lower income: the average within-firm earnings growth, which consistently averaged around 8% in periods leading up to the reform, dropped by more than 50 percentage points in 2009. Third, there was significant income shifting towards the sole proprietorship business form: the number of partnership owners reporting positive sole earnings went up by 18% in 2009. Fourth, there was no discernible income shifting towards the corporate business form as only a few partnerships became corporations even after the tax disadvantage of doing so was largely removed. Using the research designs, I translate these responses into behavioral elasticities and compute the welfare cost of the reform. The responses created by the reform are so large that by the third year of its introduction the government was collecting less revenue than it would have without the tax increase. This implies that the new, flat tax rate of 25% was on the wrong side of the Laffer curve and would not have been optimal under any social preferences.

Exploiting retroactive applicability, I characterize the nature of the observed responses. I argue that the predominant mechanism underlying the intensive margin response – the tax-driven changes in reported earnings conditional on participation – was tax evasion. It is because the 2010–11 responses, which potentially conflate both real and evasion margins, were not different from the 2009 response, which captures tax evasion mostly. I am, however, less certain whether the extensive margin response – the tax-driven changes in the number of tax-paying firms – captures firms exiting into informality or firms shutting down completely. It is because the two extensive margin choices I observe in the data – firms reporting zero earnings or disappearing completely after the reform – are potentially consistent with both explanations. However, considering the structure of social insurance in Pakistan, in particular that the owners of the exited firms would not be eligible for any government assistance and would have to work to finance consumption, it is highly likely that the extensive response in large part reflects exit into informality.[4]

One key assumption underlying the sufficient statistics approach commonly used for welfare analysis in the tax responsiveness literature (Feldstein, 1999; Chetty, 2009b; Saez et al., 2012) is that the tax change

does not generate significant externalities such as income shifting. In contexts where this assumption is unreasonable, it is necessary to either estimate the consequences of the tax change on other bases directly or to adjust the welfare measure on the basis of some assumption on these consequences. This paper takes the former approach. The Pakistani context permits simultaneous identification of earnings responses and fiscal externalities arising out of the tax increase. I, therefore, estimate one negative – spillover effects on the value-added tax base – and two positive – income shifting towards sole proprietorships and corporations – externalities created by the reform separately, and incorporate them into the welfare computations directly.

This paper contributes to three different strands of literature. First, it adds to the literature that estimates behavioral responses to taxation using administrative tax return data (see Saez et al., 2012 for a recent survey). Most of the existing studies in this literature focus on only one margin of response. This paper represents perhaps the first effort that identifies all important margins of firm response to taxation together. Uncovering the anatomy of response, especially its decomposition into intensive and extensive margins, is particularly important in developing countries because policies to mitigate tax evasion and encourage formalization do not necessarily overlap (see Bruhn and McKenzie, 2014 for evidence on policies to encourage firm formalization in developing economies). In addition, using retroactive applicability of the tax reform I am able to separate the real and reporting margins. Such separation is generally not feasible unless special tax variation is available (Carrillo et al., 2017 and Bachas and Soto, 2017 are two other recent studies that separate the real and reporting responses). On the methodological standpoint, this study has the advantage that the tax variation created by the reform is not correlated with the prereform earnings, and consequently it does not face the principal identification challenge faced by other studies in this line of literature, that is mean reversion (see Saez et al., 2012; Kopczuk, 2012 for a discussion on this issue).

Second, another important strand of literature estimates tax evasion and studies its relationship with the marginal tax rate (Andreoni et al., 1998; Slemrod and Yitzhaki, 2002). Due to the well-documented difficulties, only a handful of studies (such as Fisman et al., 2004; Marion and Muehlegger, 2008; Kleven et al., 2011; Best et al., 2015; Waseem, 2017) are able to identify tax evasion cleanly. Even more difficult is to pin down its relationship with the tax rate: the comparative statics of evasion with respect to the marginal tax rate are highly sensitive to modeling assumptions (Slemrod and Yitzhaki, 2002), and both the sign and magnitude of the effect are open empirical questions (Kleven et al., 2011). This paper identifies tax-driven evasion cleanly and demonstrates that at least for risk-neutral agents in a low-enforcement setting it responds positively to the marginal tax rate.

Finally, this paper is related to studies that examine the impact of taxes on business organization choice of firms (see for example Gordon and MacKie-Mason, 1997; Goolsbee, 2004; Gordon and Slemrod, 2000). None of the existing studies, however, looks at the question from a developing country perspective, where returns to different business forms could radically be different from those in rich countries.

The rest of this paper is organized as follows. Section 2 develops conceptual framework, Section 3 provides an overview of the context and data, Section 4 describes the research design, Section 5 reports the empirical results, Section 6 computes the welfare costs of the reform, and Section 7 concludes.

2. Conceptual framework

This section develops a simple model of firm behavior under imperfect enforcement to highlight the channels through which taxation affects welfare in a developing country setting. The model captures key elements of the tax environment, illustrating that increased taxation can induce firms to *(i)* reduce output, *(ii)* increase tax evasion, *(iii)*

---

[4] It is particularly true because I am able to show that migration within the formal sector – income shifting to sole proprietorships, corporations, and wage-earning sectors – is swamped by migration out of the formal partnership sector.

change business form, or *(iv)* exit into informality. I first consider the decision problem of a single firm and then extend the analysis to allow for heterogeneity.

2.1. Setup

Consider a firm that decides how much output y to produce; how much earnings e to evade; and whether to operate as a sole proprietorship, a partnership, a corporation, or an unorganized firm in the informal sector. The production and evasion possibilities offered by the four business forms are characterized by the production costs $c_j(y)$ and evasion costs $g_j(e)$, where $j \in \{s,p,c,u\}$ indexes the business form. Evasion costs include, inter alia, the expected tax and fines that would be recovered in case the evasion is detected.[5] The firm faces a perfectly elastic demand and can supply as much output as it desires at a fixed price, which has been normalized to one. If the firm decides to operate as type j, it obtains the following after-tax profits

$$\pi_j = y_j - c_j(y_j) - g_j(e_j) - T_j(y_j, e_j), \tag{1}$$

where $T_j(.)$ is the tax liability faced by the firm given by

$$T_j(y_j, e_j) = \tau_j \cdot [y_j - \mu_j c(y_j) - e_j]. \tag{2}$$

The tax liability depends on tax rate τ_j and reported earnings $z_j \equiv y_j - \mu_j c(y_j) - e_j$, where $\mu_j \in [0,1]$ represents the fraction of the production costs that are allowed to be deducted from revenue for tax purposes. The parameter is allowed to vary across business forms because the tax rules governing the deduction of costs are slightly different for the corporate and noncorporate forms. The firm makes its output and evasion choices by maximizing after-tax profits, producing the following first-order conditions

$$c_j'(y_j) = \frac{1 - \tau_j}{1 - \mu_j \tau_j} \tag{3}$$

$$g_j'(e_j) \geq \tau_j. \tag{4}$$

These conditions under the strict convexity of the two cost functions imply that optimal output $y_j(\tau_j)$ is a decreasing and optimal evasion $e_j(\tau_j)$ is a nondecreasing function of the corresponding tax rate τ_j.[6]

I assume that the firm has full information on the production and evasion costs so that it can compute after-tax profits from the four options before choosing its business form. It, therefore, decides to operate as type j if the profits from doing so are at least as large as those from the other options j'

$$\pi(y_j(\tau_j), e_j(\tau_j)) \geq \max_j \{\pi(y_{j'}(\tau_{j'}), e_{j'}(\tau_{j'}))\} \quad \forall j' \neq j. \tag{5}$$

Intuitively, the attractiveness of a business form to the firm is determined by its ability to produce and conceal earnings of the given type. For example, it may decide to operate in the informal sector if $g_u(e) \approx 0$ and $c_u(y) \approx c_j(y); \forall j \neq u$, which could be the case for a small, labor-intensive firm. On the other hand, it may decide to operate as a corporation if $c_c(y) \lll c_j(y); \forall j \neq c$, which for example could be the case if the firm stands to gain a lot from issuing capital in its own name. It is important to note that, in distinction to the output and evasion choices,

the firm's choice of business form is a function of all tax rates and therefore may change if any of the rates changes.[7]

2.2. Heterogeneity

The decision problem of a single firm carries over naturally to a setting with many heterogeneous firms. Each firm is characterized now by a vector $\boldsymbol{\theta}$ of firm-characteristics, which at the minimum includes the two cost functions $c_j(y)$ and $g_j(e)$ that determine its ability to generate and conceal income of type j.[8] I assume that there are a continuum of firms of measure one that draw idiosyncratic $\boldsymbol{\theta}$ from a smooth distribution $F(\boldsymbol{\theta})$. These firms make their output, evasion, and business form choices according to conditions (3)–(5). In heterogeneous setting, condition (5) implicitly defines the set of values for $\boldsymbol{\theta}$ that induces firms to choose type j conditional on the vector of tax rates $\boldsymbol{\tau}$ chosen by the government[9]

$$M_j(\tau) = \{\theta: \pi(y_j(\tau_j, \theta), e_j(\tau_j, \theta)) \geq \max_j \{\pi(y_{j'}(\tau_{j'}, \theta), e_{j'}(\tau_{j'}, \theta))\} \ \forall j'$$

$$\neq j. \tag{6}$$

Thus, under the assumption that ties occur with zero probability, a fraction ξ_j of firms operate as type j

$$\xi_j(\tau, \theta) = \int_{\theta \in M_j(\tau)} dF(\theta). \tag{7}$$

The smooth distribution of $\boldsymbol{\theta}$ among firms in this way translates into smooth distributions of output and evasion within each set $M_j(\tau)$. The conditions (3)–(5), therefore, should be viewed as a mapping that for a given tax rate τ transforms $F(\boldsymbol{\theta})$ into four empirical distributions of reported earnings $F(z_j(\tau))$.

2.3. Welfare

The principal interest of this paper is to investigate how taxation affects firms' output, compliance and business-form choices. Specifically, the empirical application considers a reform that increases the tax rate on partnership earnings (τ_p) and investigates its impacts on the firm choices. To characterize the normative implications of these choices, I define social welfare simply as the sum of private surplus and public revenue

$$W(\tau, \theta) = \sum_j \int_{\theta \in M_j(\tau)} \{\pi(y_j(\tau_j, \theta), e_j(\tau_j, \theta)) + T(y_j(\tau_j, \theta), e_j(\tau_j, \theta))\} dF(\theta). \tag{8}$$

Denoting aggregate partnership earnings by

$$Z_p(\tau, \theta) \equiv \int_{\theta \in M_p(\tau)} \{y_p(\tau_p, \theta) - \mu_p c(y_p, \theta) - e_p(\tau_p, \theta)\} dF(\theta), \tag{9}$$

the change in social welfare caused by a small increase in tax rate $d\tau_p$ can be written as

---

[5] I assume that the costs of evasion $g_j(e)$ are predominantly resource costs and not just transfers across agents (see Chetty, 2009a for the distinction between the two). The assumption is motivated by the observation that in developing economies tax evasion is typically achieved at the cost of a loss in productivity. The productivity loss occurs from activities needed to hide real earnings from government, such as operating in cash.

[6] Note that the right hand side of Eq. (3) is decreasing in τ_j as long as $\mu_j < 1$. With $\mu_j = 1$, production costs are fully tax-deductible and the tax system does not distort output decision of the firm. The Pakistani tax code does not permit complete adjustment of costs. Specifically, the owners of sole proprietorships and partnerships are expressly barred from claiming wages from the firm. Similarly, corporate firms are not allowed to claim a deduction for the depreciation of tangible assets over and above the prescribed rates. Consistent with these provisions, I assume that μ_j is bounded from above by one.

[7] A related point is that while the firm's output and evasion choices depend on the corresponding marginal tax rate, its business form choice depends on the average tax rates. In this model, I assume proportional taxation implying that the marginal and average tax rates are the same. In the Pakistani context, this assumption is not restrictive, as the tax system is proportional within brackets.

[8] Note that the production and evasion costs differences across the organizational forms primarily arise from the inherent features of the organizational form. For example, complementarity in skills of entrepreneurs makes partnership between them more productive than sole proprietorships. Similarly, evasion is more feasible in single-owner firms than in multi-owner ones. But such differences could also reflect entrepreneur-characteristics correlated with the choice of organizational form. For example, high-evasion-propensity entrepreneurs might be attracted towards sole proprietorships because of the greater ease they offer to evade. In the model, the differences arising from both sources are captured in a reduced-form way through the parameter vector $\boldsymbol{\theta}$.

[9] The treatment of firms' discrete choice of choosing a business form here follows the standard random-coefficients, discrete choice models (see for example Nevo, 2000).

$$\frac{dW}{d\tau_p} = -\frac{\tau_p}{1-\tau_p}[\underbrace{\varepsilon_p}_{\text{Intensive Margin}} + \underbrace{\eta_p}_{\text{Extensive Margin}} + \underbrace{\hat{\eta}_{sp} + \hat{\eta}_{cp}}_{\text{Income Shifting}}]Z_p. \tag{10}$$

The above expression exploits the envelope condition, noting that the tax change has no first-order impact on private welfare because it is already at the optimum. In addition, the direct effect of the tax increase on firm profits cancels out, as the welfare criterion implicitly assumes that the additional revenue is returned to firms in a lump sum fashion. This effectively reduces the welfare costs of the reform to its impact on public revenue only, which can be broken down into the following three terms.

2.3.1. Intensive margin response
This term reflects that partnerships might reduce output and/or increase evasion following the tax rate rise, according to conditions (3) and (4). These two effects act in the same direction, reducing government revenue from the tax base. The aggregate effect is proportional to the elasticity of reported partnership earnings ε_p and can be broken down further into the two underlying margins

$$\underbrace{\varepsilon_p}_{\text{Intensive Margin}} = \underbrace{\sigma_p \varepsilon_p^y}_{\text{Real Response}} + \underbrace{(1-\sigma_p)\varepsilon_p^e}_{\text{Evasion Response}}, \tag{11}$$

where ε_p^y is the elasticity of real partnership base (the first two terms inside the integral in Eq. (9)); ε_p^e is the elasticity of tax evasion by partnerships; and σ_p is the ratio of real and reported partnership base. These elasticities are aggregate elasticities defined with respect to the net-of-tax rate $1-\tau_p$, and given that firms in this model are heterogeneous are income-weighted averages of the corresponding firm-level elasticities.

2.3.2. Extensive margin response
This term captures firms that exit the partnership base after the rate τ_p goes up. For these firms, the net value from operating as partnership $j=p$ was larger than the other options $j'\in\{s,c,u\}$ at the prereform rates but not at the post-reform rates. Some of these firms would change their business form to sole proprietorships or corporations, while the rest would disappear into the informal sector. The size of the effect depends on the aggregate extensive margin elasticity defined as $\eta_p \equiv \frac{(1-\tau_p)}{\xi_p(\tau)}\frac{\partial\xi_p(\tau)}{\partial(1-\tau_p)}$, which as earlier is an income-weighted average of the firm-level elasticities.

2.3.3. Income shifting
This term captures partnerships that change their business form after the reform. Such income shifting offsets revenue loss from the extensive response mentioned above. The size of the effect depends upon the two income shifting elasticities $\hat{\eta}_{kp} \equiv \frac{(1-\tau_p)}{\xi_k(\tau)}\frac{\partial\xi_k(\tau)}{\partial(1-\tau_p)}$; $k\in\{s,c\}$, where a different notation for the elasticities ($\hat{\eta}$) is to emphasize that they are aggregated using revenue rather than income weights. The revenue weighting here reflects that the shifted earnings face potentially different tax rate in the new tax base. In the extreme case where all partnerships that exit reappear as sole proprietorships or corporations contributing the same revenue as earlier, the income-weighted elasticity η_p and the sum of the two revenue-weighted elasticities $\hat{\eta}_{sp} + \hat{\eta}_{cp}$ will cancel each other and there will be no change in the government revenue or social welfare.

2.4. Spillover effect on the VAT base

The analysis so far has ignored one important feature of the tax environment that a subset of firms also remit VAT on their sales. The income-tax-driven changes in firm behavior would impact government revenues from the VAT base as well, increasing the costs of the reform above those given by formula (10). In Appendix A.2, I show how I incorporate this fiscal externality into the welfare computations.

Three features of the framework above are idiosyncratic to the Pakistani setting and need to be emphasized. First, I do not explicitly model a firm's choice to shut down completely. This is because the data does not allow me to distinguish between firms producing zero output and firms operating in the informal sector. The margin exit into informality in this paper therefore includes *real* exit.[10] Second, the welfare analysis here focuses solely on the revenue effects of the tax reform, ignoring the impacts on welfare operating through the input and output markets. For example, firms that leave the formal sector might lay off workers, creating additional welfare losses that are not captured in formula (10). I do not observe firms' interactions in these markets and therefore cannot take these into account. And finally, the framework I use is static in nature and abstracts from dynamic decisions such as investment. It is an appropriate framework for the Pakistani setting because responses of relatively small, less capital-intensive firms over a shorter horizon of up to three years are considered.

3. Institutional background and data

This section describes institutional features of the Pakistani setting, focusing in particular on changes in the tax treatment of corporate and noncorporate firms in the country between 2006 and 2011.

3.1. Taxation of firm profits in Pakistan

Consistent with the international practice, Pakistan has two separate regimes for the taxation of corporate and noncorporate firms. Profits of noncorporate firms – sole proprietorships and partnerships – are taxed through the personal income tax schedule. In periods prior to the reform, a single tax schedule was applicable to earnings of both types of firms. It consisted of fourteen brackets with a fixed *average* tax rate, varying progressively from 0% at the bottom to 25% at the top, assigned to each bracket. The reform, announced on June 6, 2010, replaced this schedule with two different tax systems. For partnerships, a new flat-tax scheme involving a tax rate of 25% with no exemption threshold was introduced. The change was applied retroactively from July 1, 2009, so that partnership earnings corresponding to tax year 2009 and onward were subject to the new tax rate. For sole proprietorships, the progressive tax schedule was maintained, but the number of brackets was reduced from fourteen to six and the bracket cutoffs were moved. The new schedule was applied prospectively from July 1, 2010, so that sole-proprietorship earnings corresponding to tax year 2010 and onward were subject to the new tax rates. The new schedule generally maintained the prereform rates but the movement of the bracket boundaries meant that sole proprietorships in some areas of the income distribution experienced a slight reduction in tax rates.

In contrast to noncorporate firms, profits of corporate firms in Pakistan were always taxed at a flat rate of 35%. Small companies defined as corporations which *(i)* register after June 2005, *(ii)* have no more than 250 employees, *(iii)* have annual sales up to PKR 250 million,[11] *(iv)* have paid-up capital up to PKR 25 million, and *(v)* have not been formed by the splitting up or reconstitution of a company already in existence were allowed a concessionary tax rate of 20%. Such small companies comprise less than 15% of the corporate sample. During the period 2006–2011, the standard tax rate on corporate earnings stayed unchanged at 35%, but the rate applicable to small companies was increased to 25% from 2010.

---

[10] Conflating these two margins, however, is not as restrictive as it seems, especially if we take into account the structure of social insurance in Pakistan. The country has only a small means-tested income transfer program, targeted to extreme poor. Since income tax exemption threshold is set relatively high, the owners of firms dropping out of the formal sector would not be eligible for any government support and would have to work to generate consumption. Thus, as long as the costs of operating in the formal sector are not too high, it is natural to expect that firms dropping out of the formal sector would choose to operate informally rather than shut down.

[11] The PKR-US$ exchange rate hovered between 60 and 90 during 2006-11.

A: Tax Rates

- —— Partnerships (2009–11)
- ········ Sole Proprietorships (2006–09) & Partnerships (2006–08)
- - - - Sole Proprietorships[1] (2010–11)
- – – – Corporations[2] (2006–2011)

B: Average Tax Liability

—◆— Partnerships   —■— Sole Proprietorships   – ◆ – Corporations
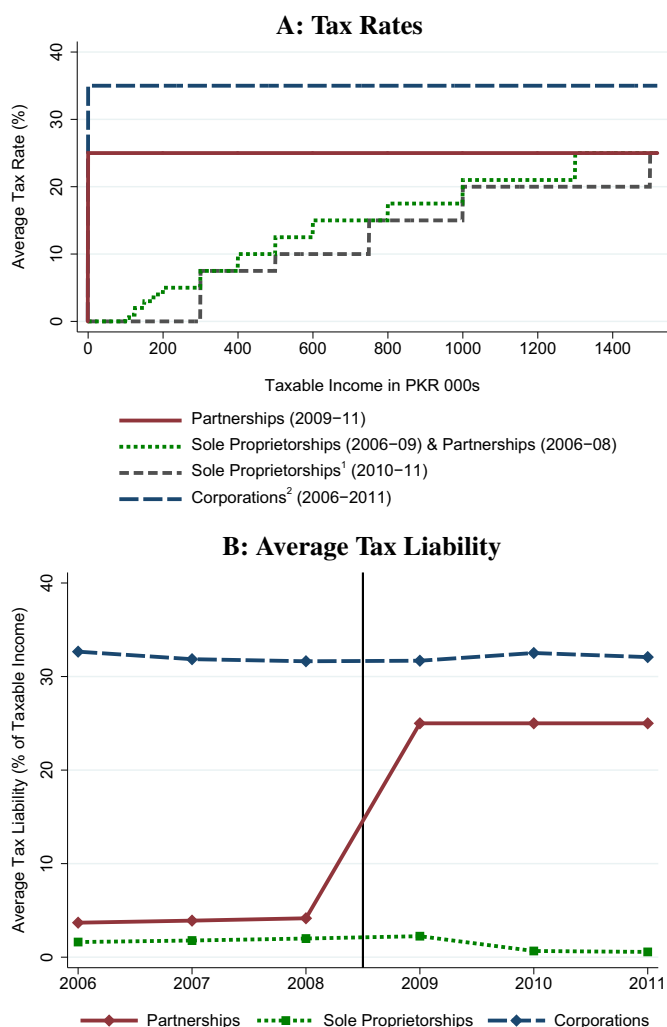
Fig. 1. Tax variation created by the reform. 1. Exemption threshold for sole proprietorships in 2011 was PKR 350,000. 2. For small corporations the tax rate was 20% which increased to 25% from 2010 (see Section 3.1 for details). Notes: The figure displays tax variation created by the reform. Panel A plots the tax rates applicable to the three types of firms from 2006 to 2011. The Pakistani tax code prescribes average rather than marginal tax rate in a given bracket of income, and all curves accordingly show the average tax rate as a function of annual taxable income. Taxable income is shown in thousands of Pakistani Rupees (PKR). The PKR-USD exchange rate was about 60 in 2006 and increased to around 90 in 2011. Panel B plots the evolution of average tax liability experienced by the three types of firms from 2006 to 2011. The average has been estimated on the actual sample of filers in each year and has been defined as the aggregate tax liability as a percentage of the aggregate taxable earning of the type of firms in the year.

Fig. 1 illustrates the tax variation created by the reform: the top panel compares the pre- and post-reform tax rates, and the bottom panel plots the evolution of average tax liability experienced by the three types of firms.[12] As a result of the reform, the average tax liability faced by partnerships firms quintupled, increasing from 5% to 25% of earnings in 2009. In contrast, the average tax liability faced by sole proprietorships remained unchanged up to 2009 but decreased slightly from 2010 when the revisions to the existing tax schedule became operational. The average corporate tax liability stayed almost the same throughout the sample period.

3.2. Registration and filing rules

All firms with earnings above the exemption cutoff are required to register with Pakistan's tax authority, the Federal Board of Revenue (FBR). On registration, partnerships and corporations are assigned a unique tax identifier. Sole proprietorships, on the other hand, are considered indistinguishable from their owners: the firm and owner share the identifier and file a common tax return. As long as they are registered, firms are required to continue filing returns even if their income falls below the exemption cutoff.

A firm can change its business organization at any time. If a partnership decides to become a corporation, it needs to get itself registered as a company with the Security and Exchange Commission of Pakistan (SECP). Before such incorporation, the firm has to re-register with the FBR as a company and to get a new identifier. Incorporation is a costly process, as in addition to paying a fee, which begins from PKR 5000 and increases with the issued capital, the firm is required to register with a host of other departments and regulatory institutions. In contrast, if a partnership breaks up and the owners desire to continue the divided business as sole proprietors, no regulatory approval is needed. The owners can do it on their own, reporting earnings of the new sole proprietorships in their personal tax returns. These rules have important implications for identifying income shifting from partnerships to the other business forms. Specifically, income shifting to corporate firms, if it happens, would leave two markers: *(i)* the entry of new companies would increase because of the fresh registration requirement, and *(ii)* former partnership owners would start reporting positive dividend income in their personal returns. Compared to this, income shifting to sole proprietorships would manifest itself only in the personal returns of the former partnership owners.

3.3. Data

I use administrative data from the FBR that include the universe of income tax returns filed in 2006–2011 and a set of taxpayer characteristics reported at the time of registration. The tax return dataset contains variables corresponding to line items reported on the return form, including a brief profit and loss account, the decomposition of taxable income by source, and tax computations. The registration dataset includes individual and firm characteristics, such as date of registration, industry, and region. Since July 2009, electronic return filing is mandatory for all firms other than small sole proprietorships. Consequently, most of the 2008–2011 returns used in this study have been filed electronically.[13] The rest of the returns were filed at designated bank branches and were fed into computers by an IT firm distinct from the FBR. Throughout the period covered by this study, the FBR has been using the data for automated processing and payment of VAT and income tax refunds, which has ensured that the data were kept updated and relatively free from errors.[14]

Table 1 presents the summary statistics of the data for the baseline year 2008. All empirical results in this paper, unless otherwise stated, are based on the analysis sample, which contains firms that have base period income (z_{it}) in the range [0 650,000]. The analysis sample contains around 95% of all partnerships in the sample (row 2 of the table). I exclude firms in the rest of the income distribution because they experience relatively smaller tax changes and the density of tax filers in the region is too thin to estimate responses credibly.

Expectedly, annual sales and earnings of partnerships (rows 1 and 4) are on average lower than those of corporations and higher than those

---

[12] The average tax liability has been estimated on the actual sample of filers in each year and has been defined as the aggregate tax liability as a proportion of the aggregate taxable earning of the type of firms in the year.

[13] Returns for a year *t* are filed in the September of year *t* + 1. The electronic-filing provision, therefore, applies to all 2008–2011 tax year returns.

[14] It is important to emphasize that the Pakistani tax system is based on the principal of self-assessment, meaning thereby that all filed returns are considered final unless selected for audit. Each year, the tax administrations audits a small sample of returns. I, however, do not observe the incidence or outcome of the audits.

Table 1
Summary statistics.

	Full sample			Analysis sample		
	Partnerships (1)	Sole props. (2)	Corporations (3)	Partnerships (4)	Sole props. (5)	Corporations (6)
Outcomes						
1. Taxable Income	442,114	350,234	83,428,856	223,319	147,171	200,958
	(1,829,383)	(20,390,724)	(1,173,785,216)	(124,759)	(74,017)	(175,916)
	[198,100]	[125,000]	[740,074]	[185,000]	[125,000]	[147,407]
2. Number of Firms						
Taxable Income > 0	21,319	373,279	5122	19,357	365,686	2461
Taxable Income = 0	16,551	118,573	12,597	16,551	118,573	12,597
Characteristics						
3. Annual Sales	32,512,930	4,768,342	806,324,032	14,791,702	3,838,501	50,771,604
4. Tax Liability	68,307	55,154	29,173,054	13,149	4436	63,938
5. Age	4.08	7.29	6.75	3.95	7.25	5.75
6. Electronic Filer	0.56	0.05	0.99	0.55	0.05	0.98
7. VAT Registered	0.24	0.06	0.52	0.21	0.06	0.37
8. Round Filer	0.41	0.67	0.02	0.44	0.68	0.03
9. Buncher	0.28	0.36	–	0.31	0.37	–
10. Dominated	0.03	0.02	–	0.03	0.02	–
11. Revised Return	0.01	0.00	0.05	0.01	0.00	0.04
12. Large City	0.39	0.37	0.76	0.37	0.37	0.71
13. Withholding Agent	0.14	0.01	0.92	0.09	0.01	0.90

Notes: This table presents descriptive statistics of the data for the baseline year 2008. Analysis sample contains firms with earnings in the range [0 650 K], whereas the full sample contains all firms with nonnegative earnings. The first row compares the mean, standard deviation, and median of taxable income reported by the three types of firms. The standard deviation and median are in parenthesis and square brackets respectively. Rows 3–13 compare the mean of the firm characteristic variable across the three types of firms. Annual sales and tax liability are reported in PKRs. Age is defined as the number of years a firm has been registered with the FBR. Round filer is defined as a firm which reports earnings in exact multiples of thousands. It is generally considered a good indicator of the quality of record keeping within the firm. Buncher and Dominated are the sole proprietorships and partnerships with earnings within the bunching and strictly dominated regions around the notches in the baseline tax system (2006–08) as defined in Kleven and Waseem (2013) . Revised return indicates that a firm files a revised return for 2008 to rectify any mistakes in the original return. Large City indicates that a firm has its head office in one of the three big cities of Pakistan — Lahore, Karachi, and Islamabad. The detailed description of the rest of the variables is provided in Appendix A.1.

of sole proprietorships. Similarly, in terms of other characteristics that determine a firm's propensity to comply with tax laws (rows 6–11) partnerships lie in between the other two business forms. This is particularly helpful, as having a control group on either side of the compliance scale acts as a natural robustness check on the internal validity of the difference-in-differences estimates. Appendix Fig. A1 compares the industry, geographic, and size distribution of firms. While the industry and size distribution of firms is fairly similar, the geographic distribution is not: corporations are mostly located in the top three cities of Pakistan, whereas partnerships and sole proprietorships are distributed symmetrically throughout the country. To account for the geographic disparity, I also report results from specifications that include the region fixed effects.

3.4. Was the reform anticipated ?

I use retroactive application of the tax increase to characterize the nature of the observed responses (real response vs. tax evasion). Such characterization requires that the reform was not known before its official announcement in June 2010. To assess this, Fig. 2 compares the entry of new partnerships and corporate firms in Pakistan. For this particular question looking at the entry is helpful as the data are available at a daily frequency and given the large size of the tax shock the time around which the tax increase became known can be identified from a break in trend. Panel A plots the raw data, aggregating the entry to a monthly frequency. Panel B plots the coefficient and 95% confidence interval from a difference-in-differences regression on the two entry series in Panel A. Of the 47 preannouncement months considered here (July 2006 to May 2010), the DD coefficient is statistically insignificant in 33 months, including the month immediately preceding the announcement. By contrast, the coefficient is negative and statistically significant in all 34 post-announcement months. The plots thus show that partnership and corporate entry were on reasonably parallel trends in periods leading up to the reform and that it was only after June 2010 that the partnership entry began deviating from the corporate entry systematically. Appendix Fig. A2 repeats the analysis

using sole proprietorships as control, reinforcing the conclusion that the reform was not anticipated.[15] This, in fact, should not be surprising as the Pakistani authorities are very secretive about tax changes, fearing that taxpayers might shift activity across time or entities to minimize their tax payments.

3.5. Income shifting costs

Eq. (5) shows that a firm's business form choice is a function of its ability to produce and conceal earnings of a given type, captured by the two cost function – $c_j(.)$ and $g_j(.)$ – and the three marginal tax rates $\tau_j; j \in \{s,p,c\}$. Given that in the Pakistani setting the three marginal tax rates are in general not equal, a firm's choice of reported earnings and organizational form can be used to infer its productivity in generating reported earnings of the given type. To see this, consider a partnership that produces y units of output and evades e units of income, facing a tax liability of $\tau_p [y - \mu_p c_p(y) - e]$. Using tax rules and data from returns filed by its owners, I am able to compute the firm's counterfactual tax liability had it operated as a sole proprietorship choosing the same level of output and evasion. Under the assumption that the parameter μ_j does not vary across the two business forms, the difference represents the minimum earnings boost that the firm experiences from operating as a partnership. Since the firm would lose at least this much of profits if it decides to become a sole proprietorship, the difference provides a lower bound on income shifting costs in the neighborhood of the optimum.

Fig. A3 plots the distribution of these costs for partnerships in 2006–11. The histograms show that these costs are generally quite large, suggesting that the change of organizational form is not a trivial

---

[15] For sole proprietorships, however, I observe the date of registration only if the firm files a return, as they are not required to register separately from their owners. The analysis in the figure, accordingly, is limited to the subset of firms which file tax return in the sample period. For this reason, the two entry series in the figure decline mechanically over time and are more noisy. Nevertheless, the results are consistent with those from corporate firms as control.
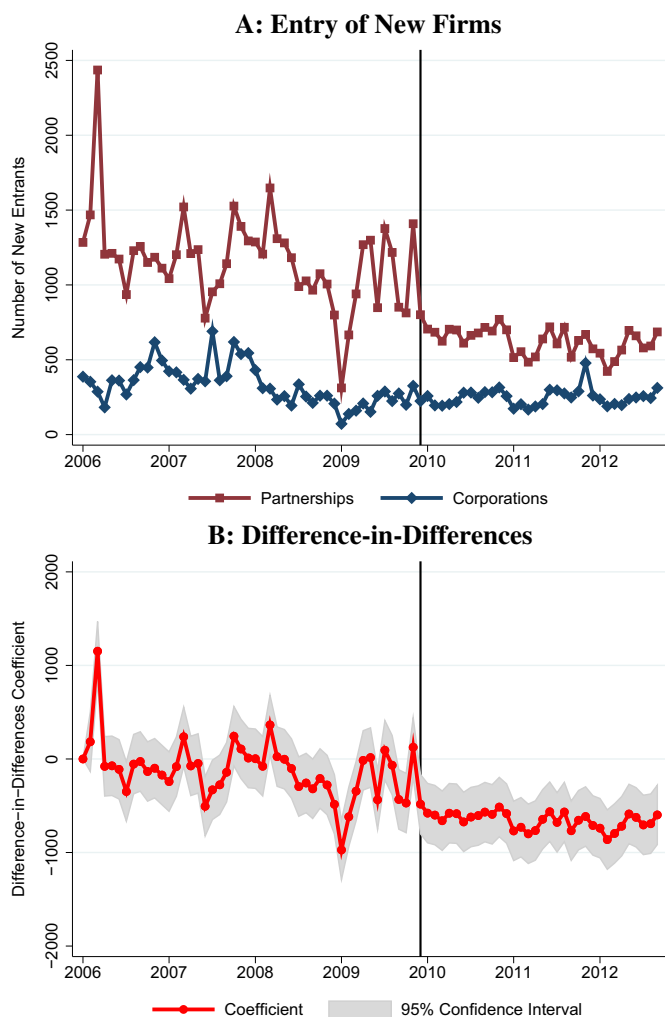
Fig. 2. Was the reform anticipated? Notes: The figure investigates if the reform was anticipated before its official announcement on June 6, 2010. Panel A of the figure compares the entry of partnerships with that of corporations from July 2006 to March 2013. Each dot on the two curves represents the number of firms that get registered with the tax authority in that particular calendar month. Year *t* on the x-axis indicates the first month (July) of the tax year *t*. Dashed vertical line demarcates June 2010. Panel B of the figure plots the coefficients from a difference-in-differences regression on the two series in Panel A. Each dot on the solid curve shows the coefficient for the particular month, indicating the additional entry of partnerships in the month relative to corporations. The gray area plot shows the 95% confidence interval around the coefficient.

decision for a firm and involves a real loss in productivity.[16] Note that I cannot conduct similar analysis for partnerships vs. corporations, as the cost deduction rules (μ_j) are different for the two business forms. The variation in μ_j means that I am unable to compute the counterfactual tax liability of a partnership if it operates as a corporation from the available data.

4. Research design

This section describes the difference-in-differences research designs

I use to estimate the parameters in formula (10). The idea behind the research designs is to compare partnership outcomes with sole proprietorship and corporate outcomes over time to isolate the tax-driven effects. The claim here is not that a firm's business organization is randomly assigned; it is rather that the outcomes would have evolved similarly had the tax rates not changed.

4.1. Intensive margin

To obtain the intensive margin elasticity, I estimate the following model

$$\Delta \log z_{it} = \alpha + \beta \, Partnership_i + \varepsilon \, \Delta \log(1 - \tau_{it}) + X_i \, \delta + \lambda_t + u_{it},$$

(12)

where *i* and *t* index firms and tax years; $\Delta \log z_{it}$ is within-firm log change in reported earnings from period $t - 1$ to *t*; $Partnership_i$ is a dummy indicating that *i* is a partnership, $\Delta \log(1 - \tau_{it})$ is within-firm log change in net-of-tax rate from period $t - 1$ to *t*; X_i are a set of controls; and λ_t are year fixed effects. To address the potential endogeneity of tax rate to the choice of reported earnings, I use tax variation created by the policy reform only and instrument $\Delta \log(1 - \tau_{it})$ in the first-stage with the double-difference inter-action term $Partnership \times Post_{it}$, where $Post_t$ is a post-reform period indicator. I estimate the model using sole-proprietorships and corporations as controls, reporting the results from the alternative specifications separately. The baseline specification does not include any controls, but I show that the results are robust to including industry, region, and tax office fixed effects.[17] All estimates are weighted by income so that the elasticity estimate corresponds to the parameter ε_p in formula (10).

There are three potential threats to identification in this setting. First, reported earnings might not be on a common trend in the treatment and control groups. Second, the composition of the sample might change in the post-reform periods in a way creating a correlation between the double-interaction and error terms. Third, the control outcomes might also be affected by changes introduced by the reform. I take the following precautions and/or conduct robustness checks to rule out these concerns.

I present three pieces of evidence supporting the common trends assumption. First, I show visually that the preexisting earnings trends were parallel in the treatment and two control groups. In fact, the trends were so stable and flat that even the time-series estimates are credible in this setting. Second, I supplement all regression-based estimates with placebo analysis, pretending that the reform took place one year earlier than it actually did. Third, I illustrate that the result are not affected when the year fixed effects are replaced by linear, separate linear, and industry-specific time trends.

I also estimate Eq. (12) on balanced-panel samples. These samples include only firms which report positive earnings in all years considered in the analysis. They, thus, do not allow entry and exit, holding the composition of the treatment and control samples fixed for the entire period of estimation. The results from the balanced-panel samples are always comparable to those from the complete samples, suggesting that *(i)* concerns from a change in the composition of the sample caused by endogenous entry and/or exit of firms are not important in this setting, and *(ii)* the intensive margin responsiveness estimated from Eq. (12) is broadly representative of the responsiveness in the population.[18]

Finally, I take two precautions to ensure that the control outcomes are not contaminated directly or indirectly by the tax changes. First, to

---

[16] Theoretically, the income shifting costs could reflect either that firm operating as partnerships are more efficient in producing output $c_p(y) < c_s(y)$ or that they are able to hide income more easily $g_p(e) < g_s(e)$ relative to if they operate as a sole proprietorships. While I am unable to break down the costs into these two components, the fact that partnerships have attributes that are generally negatively associated with tax evasion – for example, on average they are larger, have higher earnings, have greater fraction of their earnings reported by third parties, are more likely to be electronic-filers, and respond less aggressively to tax incentives (Table A20) – suggests that the income shifting costs reflect in large part the ability to produce output more efficiently.

[17] Pakistan has two types of tax offices: three Large Taxpayer Units (LTUs) and twelve Regional Tax Offices (RTOs). Including tax office fixed effects accounts for the possibility that firms administered by different offices might have been exposed to varying levels of enforcement.

[18] It is important to emphasize that firms which exit in 2009 do not feature in either of the two samples on which Eq. (12) is estimated. While the comparability of the elasticity over time and across samples provides strong evidence that these firms would have responded similarly to the other firms had they remained active, the intensive margin responses of these firm are not directly identifiable. If these firms were different from the other in terms of their intensive-margin responsiveness, the estimates from Eq. (12) would represent the responsiveness among the active firms only and not the population.

eliminate indirect effects operating through income shifting, I drop firms whose owners hold an interest in a partnership in any period from the control groups. This, however, turns out to be a careful precaution only, as such firms constitute less than 5% of the control sample and the results with or without them are indistinguishable. Second, to address the concern that a subset of control firms are affected directly by tax changes from 2010, I first estimate Eq. (12) on a period 2006–09 producing a short-run estimate of the elasticity. I then re-estimate Eq. (12) on a period 2006–2011, excluding the subset of control firms affected by the tax changes. Such exclusion is immaterial for the corporate control group, as the relevant tax change was *(i)* applicable to only around 15% of firms; *(ii)* a function of predetermined firm-characteristics; *(iii)* a tax increase, meaning that even if we ignore it the resulting bias would only push the estimates downwards. In contrast, the exclusion could be consequential for the sole proprietorship control group, as the relevant tax change was a nonlinear function of the base period income. The concern, however, turns out to be of little practical relevance, as the 2010–11 estimates from the two alternative control groups are either statistically indistinguishable from zero or economically insignificant relative the 2009 estimate.[19]

4.2. Extensive margin

I use a three-step strategy to estimate the extensive margin elasticity. The first step in the strategy is to ascertain the counterfactual number of tax filers in the post-reform periods. For this purpose, I estimate a difference-in-differences model similar to Eq. (12) using the log number of filers as the outcome variable, where I consider a firm as a filer in period t if it reports positive taxable income in the period.[20] On the basis of the fitted model, I predict the counterfactual number of partnership tax-filer in 2009–11. The difference between the predicted and actual number of tax filers represents the extensive-margin response to the reform, which can be used to compute the corresponding unweighted elasticity.

To obtain the income-weighted elasticity needed for formula (10) and to explore response heterogeneity, I go a step further and estimate the complete counterfactual distribution of partnerships earnings. This step is predicated on the observation that for a given tax system the empirical earnings distribution does not change from year to year

$$F_t(z_p|\tau) \equiv \Pr(z_{p,it} < z_p|\tau) = F(z_p|\tau) \quad \forall\, t. \tag{13}$$

This essentially implies that the macro-driven entry and exit of firms are not correlated with firm-earnings, so that even if the *number* of tax filers changes from year to year the *density* of tax filers stays the same. I present a nonparametric test of this assumption in Appendix Fig. A4. The figure plots the observed $F_t(z_p)$ for the three preintervention periods 2006–2008, showing that consistent with Eq. (13) the empirical CDF is indistinguishable across periods of no tax change. Given this stationarity of the CDF, the counterfactual distribution can be constructed by adjusting the baseline empirical distribution to have the same mass as predicted by the difference-in-differences model in the first step.

In the final step, I compare the observed and counterfactual distributions to compute the income-weighted elasticity. Before making this comparison, I strip the observed distribution of the intensive-margin responses. Intuitively, this step is necessary because firms report

lower earnings after the tax rate increase, creating a leftwards shift of the observed distribution. Partialling out the intensive response ensures that any difference between the observed and counterfactual distribution in a given area of the distribution identifies the tax-driven reduction in the number of tax filers in the area. By weighting these local extensive response estimates with the taxable income in the area, I obtain the aggregate income-weighted elasticity η_p.

I support the elasticity estimates from the strategy with the results from the following auxiliary regression

$$1(z_{it} > 0) = \Phi(\alpha + \beta\ Partnership_i + \textbf{Partnership} \times \textbf{Post}_{it}\gamma + X_i\ \delta + \lambda_t$$
$$+ u_{it}), \tag{14}$$

where **Partnership** $\times$ **Post** $_{it}$ is a vector of three interaction terms one each for 2009 to 2011, and all other variables are defined similarly to as in Eq. (12). I fit the equation using both probit and linear models on samples containing for period t all firms that file return for the period, reporting earnings in the range [0 650 K]. The coefficients on the three interaction dummies reflect how the probability to report positive taxable income changes for partnerships in the corresponding period relative to the control firms. Though the results from this exercise are *quantitatively* not comparable to those from the strategy above,[21] it permits conducting the robustness checks mentioned in the last section in a transparent, regression-based framework.

4.3. Income shifting

The registration and filing rules described in Section 3.2 imply that if a partnership becomes a corporation, its owners would begin reporting dividend income in their personal returns. Similarly, if a partnership breaks up into sole proprietorships, its owners would begin reporting sole-proprietorship income in their personal returns. I, accordingly, use the following model to identify income shifting from partnerships to the other business forms

$$1(z_{j,it} > 0) = \Phi(\alpha + \beta\ Partner_i + \eta\ \log(1 - \tau_{it}) + X_i\ \delta + \lambda_t + u_{it}), \tag{15}$$

where, $1(z_{j,it} > 0)$ is an indicator that i reports positive earnings from source j in period t, $Partner_i$ is a dummy showing that i was a partnership owner in any of the three prereform periods, $\log(1 - \tau_{it})$ is the log net-of-tax rate experienced by i in period t, and the rest of the variables have the usual interpretation. To capture the incentive for income shifting, I simulate τ_{it} for treated taxpayers as the marginal tax rate that would apply if their source j income was reported as partnership income (it is flat 25% in the post-reform periods). For control taxpayers, the tax rate variable is computed as in the previous sections.[22] The estimates are revenue-weighted so that the elasticities from the equation correspond to the two parameters, $\hat{\eta}_{sp}$ and $\hat{\eta}_{cp}$, in formula (10). I conduct the tests mentioned in the last two sections to establish the robustness of the results.[23]

The above model considers former partnership owners receiving positive dividend income in the post-reform periods as an evidence of

---

[19] An alternative strategy to estimate 2010–11 responses would have been to control for the tax changes experienced by control firms directly in the regression. This approach, however, requires that the elasticities for the control and treated firms are the same. I decide against using this approach because the evidence in Waseem (2017) shows that the elasticity generated by tax reforms that reduce the tax rate to zero is uncharacteristically large. Many sole proprietorships experience such a tax change in 2010.

[20] To ensure that the number of tax filers in the control group are not affected through income shifting, I apply the safeguard mentioned in the last section here as well, dropping firms from the control groups whose owners have been partners in a partnership firm in the prereform periods. Again, it turns out to be a careful precaution only as the number of such firms is negligible relative to the size of the control group.

[21] The extensive margin response occurs through three distinct channels: (1) reduced entry; (2) increased exit comprising firms that exit and stops filing returns; (3) increased exit comprising firm that continue filing returns but report zero earnings. While the estimates from the strategy encompass all three channels, the estimates from Eq. (14) reflect the last channel only.

[22] Throughout this paper I compute marginal tax rate as $\tau_{it} \equiv \frac{T(z_{ilt} + \Delta) - T(z_{ilt})}{\Delta}$, where $T(.)$ is tax liability and Δ represents a small increase in income (PKR 50).

[23] For consistency, I estimate Eq. (15) using both sole proprietorships and corporations as control groups. Corporate firms, however, cannot have sole-proprietorship income. Therefore, specifications that estimate income shifting to sole proprietorships using corporate firms as controls compare the propensity to report positive sole-proprietorship income by former partnership owners to the propensity to report positive *taxable income* by corporations, attributing any difference in the post-reform years to the tax increase. I present placebo estimates to establish that such comparison indeed captures the desired tax-driven impact.
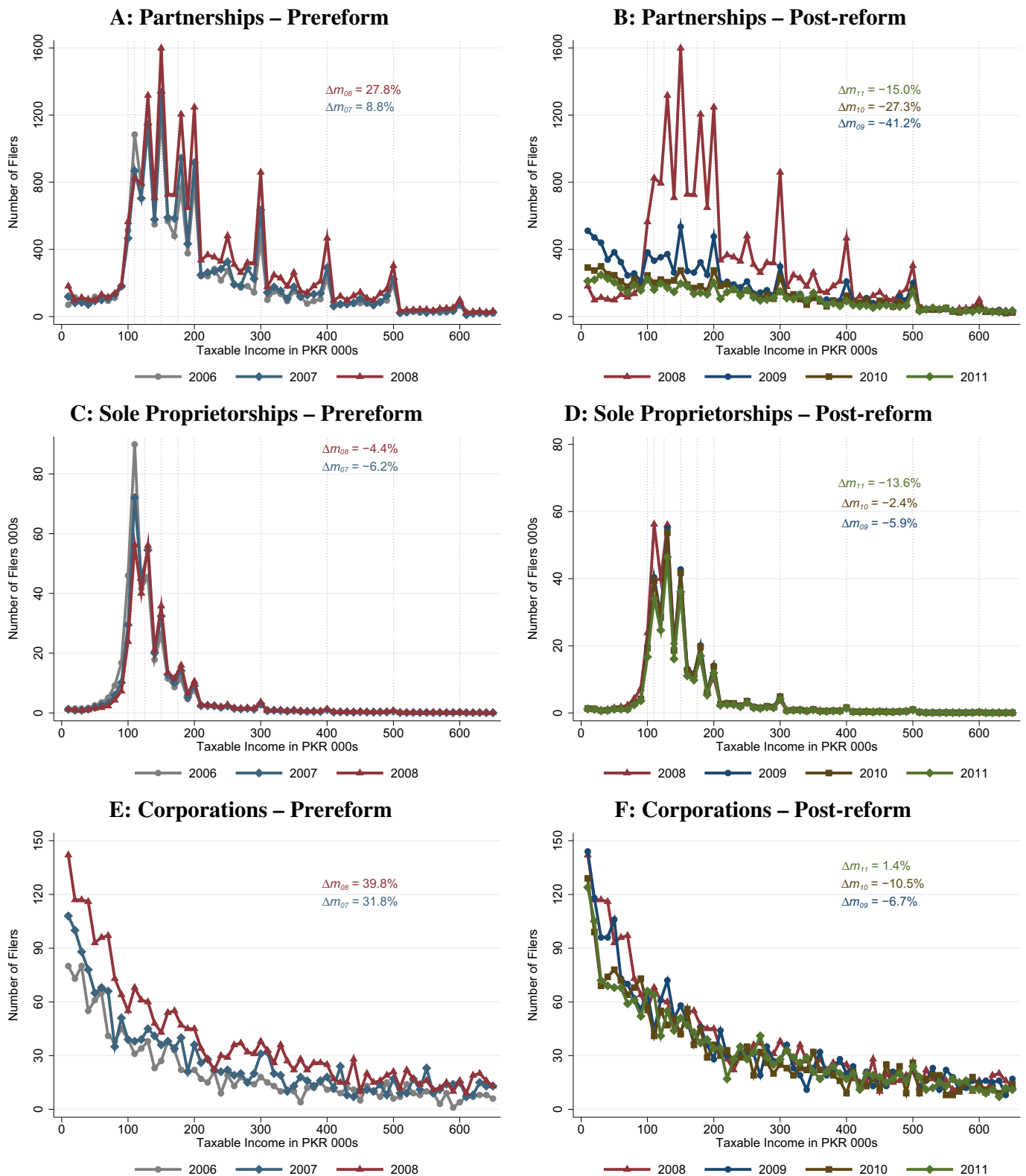
Fig. 3. Taxable income distribution. Notes: The figure compares the pre- and post-reform taxable income distributions across the three types of firms. Each dot on the curves represents the upper bound of a PKR 10,000 bin and denotes the number of firms which report earnings within that bin. The notches in the 2006–08 schedule are shown by the vertical dotted lines (Panels A–D only). In the right-hand side panels, the 2008 distribution is shown again for comparison purposes. Yearly changes in the number of tax filers are shown by Δm_t, which for year t signifies the change in the number of filers from year $t-1$ to t as a percentage of the number of filers in year $t-1$.

income shifting to corporations. The problem with this approach is that corporations may not issue dividends every year, so the model may underestimate the response. I, therefore, supplement the exercise with a nonparametric permutation test, looking at the entry of new

corporations. Since partnerships that become corporations are legally obliged to re-register, any impact of the reform on this margin can easily be detected by tracking the registration of new corporations over time. I test this using the following regression

$$Entry_t = \alpha + \beta\, t + \gamma\, Post_t + \delta\, t \times Post_t + v_t, \tag{16}$$

where $Entry_t$ refers to the number of new corporations registered in period t. This equation fits a linear trend on the pre- and post-reform entry series and tests whether this trend changes at the time of the reform. In addition to checking for the significance of $\hat{\delta}$, I compare it to similar coefficient in the placebo regressions estimated on the prereform periods only.[24] As I observe the entry of new corporations for a large number of prereform periods, I am able to generate a complete distribution of the placebo coefficient. If the reform had a significant positive impact on the entry of new corporations, the estimated $\hat{\delta}$ would lie in the upper tail of this distribution.

5. Empirical results

In this section, I first present nonparametric evidence on how the number of and earnings reported by the treated firms responded to the increase in the tax rate. Later, using the research designs detailed above I translate the responses into the behavioral parameters of interest.

5.1. Nonparametric evidence

Fig. 3A–B shows the distribution of earnings reported by partnerships over the period 2006–11. The preform plots (Panel A) illustrate two key points. First, the number of partnerships was increasing before the reform: it increased by 9% in 2007 and by 28% in 2008. Second, despite the increase in numbers the shape of the distribution was remarkably stable and did not change from one year to the other. This demonstrates that the entry and exit during the periods of no tax change are not correlated with firm-earnings, providing a direct evidence in support of the assumption Eq. (13). Fig. 3B plots the 2008 and the three post-reform distributions together, depicting the enormous impact produced by the tax increase. Not only was the increasing preform trend reversed, but also the number of partnerships started decreasing sharply after the reform. The number decreased by 41% in 2009, by another 27% in 2010, and by a further 15% in 2011. This means that within three years of the tax increase the treated tax base had shrunk to 36% of the baseline level.

In addition to the large extensive-margin response, the plots also carry the signature of the intensive margin response: The post-reform densities are higher relative to the preform densities at the bottom of the distribution (earnings < 100,000). It shows that partnerships which did not exit reported lower earnings after the reform, creating a leftwards shift of the empirical distribution.

To demonstrate that the observed responses are driven by the tax increase and not by any macroeconomic shocks, I present in Fig. 3C–F the corresponding distributions of sole proprietorship and corporate earnings. In constructing Panels C–D, I *(i)* drop sole proprietors that report any income from a partnership in 2006–11 and *(ii)* strip the 2010–11 distributions of intensive responses to the 2010 tax changes using the assumption Eq. (13). The control group earnings distributions in 2006–11 (Panels C–F) are almost on top of each other, showing no discernible change in outcomes over time. This confirms that the large-scale erosion of partnership earnings depicted in Panel B was caused by the tax increase. Appendix Fig. A5 repeats the analysis without making changes *(i)* and *(ii)* to the sole-proprietorships distributions, illustrating that the changes do not make any material difference to the conclusion.

5.2. Elasticity estimates

5.2.1. Intensive margin

Graphical evidence — Fig. 4 compares the evolution of reported earnings across the treated and control firms in the period 2006–11. The figure is based on the analysis sample, containing firms with positive earnings only *i.e.* $z_{it} \in (0\ 650\ K]$. It thus isolates the earnings response conditional on participation produced by the reform. The top panels compare the level of reported earnings, and the bottom panels display the coefficients from the difference-in-difference regressions on the two series in the top panels. Appendix Fig. A6 shows similar plots for the balanced-panel samples. Collectively, the evidence shows that the reported earnings were on parallel trends in the preform periods. They continued to evolve on the preexisting trend for the control firms but declined sharply for the treated firms in 2009. In the post-2009 period, the treated earnings began recovering, growing at almost the preform rate. This, however, means that the tax rate increase caused a lasting damage to the tax base: the level of partnership earnings was permanently lower in the post-reform periods.

Results (2009) — Table 2 reports the results from Eq. (12), restricting the sample to the period 2006–09. Starting with the baseline specification in column (1), columns (2)–(5) successively add more controls; columns (5)–(10) replace the year fixed effects with a linear time trend; Table A2 replicates the exercise on a balanced-panel sample; Table A3 permutes among the combinations of time-trend and balanced-panel specifications; and finally Table A4 repeats the analysis after reweighting the two control samples to match the treatment sample on size and industry dimensions using the DiNardo et al. (1996) method.

Two conclusions emerge from the above analysis. First, firms' earnings choices conditional on participation are extremely elastic to the tax rate: every percentage point decrease in the net-of-tax rate was associated with an almost twice-as-large drop in reported earnings. This reflects the small costs at which firms in a low tax-capacity setting are able to manipulate their earnings following a tax change. Second, the elasticity is estimated cleanly, being robust to the identification concerns mentioned in Section 4.1. Notably, the results are insensitive to *(i)* the choice of control group; *(ii)* holding the composition of the sample fixed; *(iii)* the choice of time trend (flexible vs. parametric); *(iv)* comparing firms within an industry, region, and tax-office; *(v)* allowing firms in each industry to have a separate growth-trend; *(vi)* DFL-reweighting the control samples to match the treatment sample; and *(vii)* dropping firms affected by income shifting from the control groups (Table 2 vs. Table A5). Furthermore, the placebo coefficient is statistically insignificant in almost all the 120 specifications reported in Tables 2 and A2 –A6.[25] The robustness of the results is a reflection of the stability and flatness of the preexisting partnership earnings trend. In fact, the trend was so flat that even the time-series estimates, reported in Table A6, are indistinguishable from the corresponding difference-in-differences estimates.

Results (2009–11) — Table 3 reports the results from Eq. (12), separately estimating the elasticity in the three post-reform periods. Appendix Fig. A6 display the visual analog of the results, and Table A7 presents the corresponding time-series estimates.[26] Consistent with Fig. 4, the results here show that the intensive-margin response produced by the tax reform was of an immediate and permanent nature: reported earnings underwent a steep decline in 2009 but started growing from this low base at almost the preform rate after 2009.

---

[24] Specifically, I estimate Eq. (16) on a daily frequency (t = day) for a two year time period from June 6, 2009 to June 5, 2011, defining the last year as the post-reform period. I compare $\hat{\delta}$ from this regression against that from placebo regressions, which are run identically on a two-year window with the last year defined as the post-reform periods. The estimation window for the placebo regressions starts from the period June 6, 2007 to June 5, 2009 and goes systematically back to July 1, 1995.

[25] The insignificance of the placebo coefficients across specifications shows that the reported earnings of treated firms do not change significantly from one year to the next relative to the control firms for any nontax reason including mean-reversion. This is consistent with the graphical evidence showing parallel trends in Fig. 4.

[26] Note that one important distinction between the results here and those above is that the control samples here are restricted to firms that are not impacted by the 2010 tax changes (please see discussion in Section 4.1).
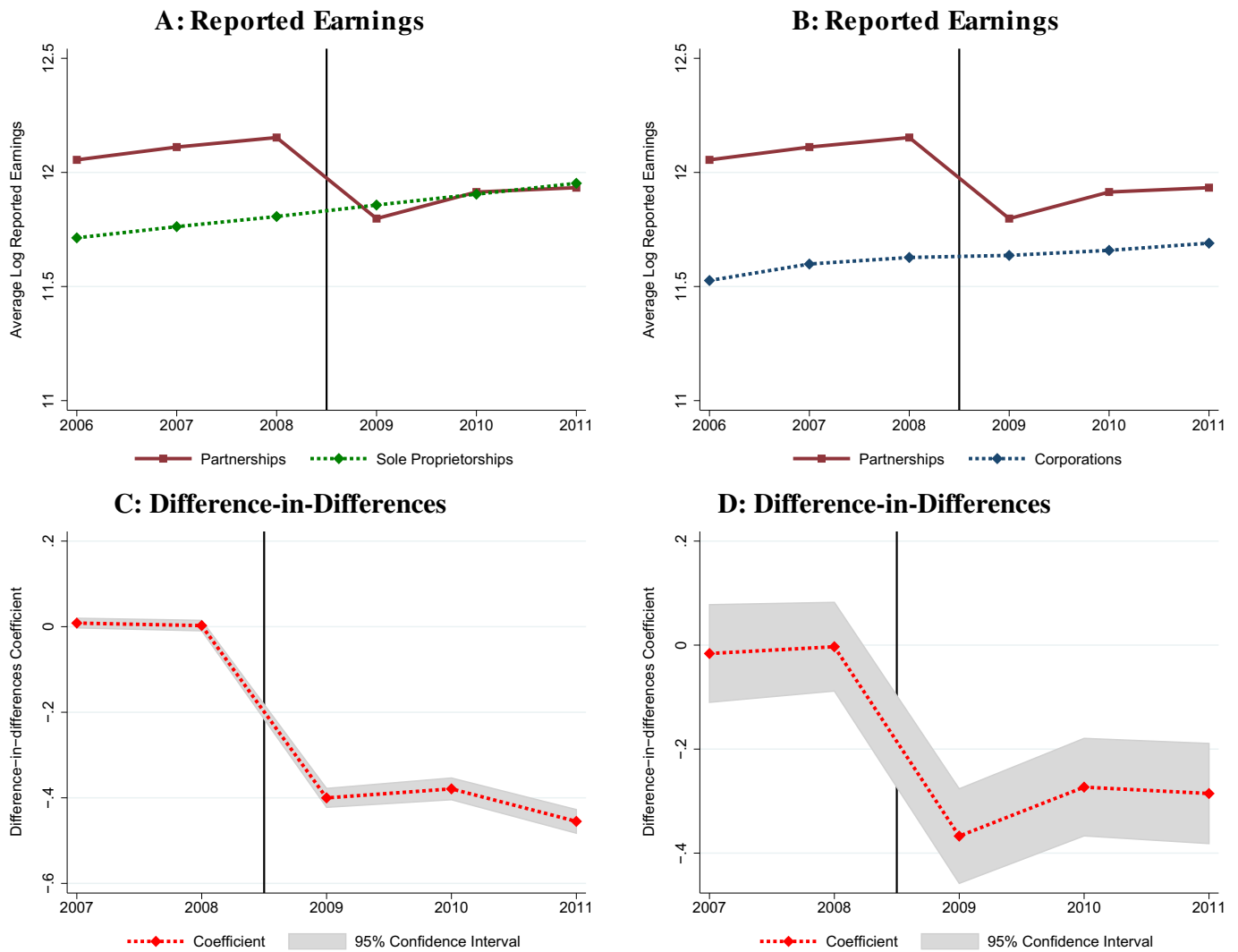
Fig. 4. Intensive margin response. Notes: The figure compares the evolution of reported earnings across partnerships and the two control groups, documenting parallel trends up to the reform and a steep decline in treated earnings thereafter. The top two panels compare the mean of log reported earnings in repeated cross-sections over time, and the bottom panel displays the coefficients from the DD regressions on the two series in the top two panels. The sample for the figure contains only firms that report positive earnings in the range $z_{it} \in (0\ 650$ K], thus isolating earnings response conditional on participation created by the reform. The solid vertical line in each panel indicates the time from which the tax changes take effect.

Reflecting this, the elasticities underlying the post-2009 responses are either statistically insignificant (Panel B) or negligible relative to the 2009 elasticity (Panel A).[27] The retroactive application imparts additional significance to the result, implying that the principal channel through which firms responded to the rate increase was tax evasion and not a real change in activity, a point I come back to in Section 5.3 of the paper.[28]

5.2.2. Extensive margin

Graphical evidence — The three steps of the strategy to estimate the extensive margin response are displayed in Fig. 5. In the first step, I

estimate the counterfactual number of tax filers in the post-reform periods. The difference-in-differences setup for this estimation is shown in the first two panels of Fig. 5. The prereform filing trend was increasing and reasonably parallel among partnerships and corporations. By contrast, the trend was decreasing for sole proprietorships, implying that the extensive margin elasticities using this group of firms as control would be underestimated. To account for this, I take two measures. First, I estimate two variants of the baseline model, allowing linear and separate linear time trends in filing. Second, I supplement the analysis with within-partnerships comparisons. The basis for this exercise is provided in Appendix Fig. A7. Panel A of the figure compares the numbers of partnerships with earnings in the range $z_{it} \in (0\ 650$ K] and $z_{it} \in [0\ 650$ K]. The difference between the two numbers for a given year represents the partnerships which report zero earnings in the year. The difference was reasonably stable in the prereform periods but grew sharply after the reform, as more firms – compelled by the increase in tax rate – shifted to zero earnings. By contrast, such difference for the two groups of control firms remained stable throughout the period 2006–11 (Panels B and C of the figure). Estimating the counterfactual number of tax filers from the two series in Panel A, thus, provides a clean and conservative lower bound on the extensive margin

---
[27] An important caveat to these results is that the sample in the three post-reform years changes because of the extensive margin response to the reform. It has the potential to introduce a bias in the estimated coefficients for the three years, although the comparability of the estimates from complete samples and balanced-panel samples largely mitigates this concern (compare LHS panels of Fig. A6 with the RHS panels, and Table 2 with Table A2).

[28] The relative insignificance of the 2010–11 responses compared to the 2009 response also suggests that firms use rudimentary, low cost technologies to achieve tax evasion, as with additional time more sophisticated evasion technologies such as keeping multiple books of accounts become feasible.

Table 2
Intensive margin elasticities (2009).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Sole proprietorships as control										
Elasticity	2.233	2.253	2.251	1.999	2.009	2.219	2.241	2.238	1.973	1.981
	(0.077)	(0.078)	(0.077)	(0.074)	(0.075)	(0.079)	(0.080)	(0.080)	(0.077)	(0.077)
Placebo	0.025	0.028	0.029	0.095	0.100	0.036	0.036	0.038	0.087	0.094
	(0.044)	(0.044)	(0.044)	(0.052)	(0.052)	(0.044)	(0.044)	(0.044)	(0.050)	(0.050)
Observations	848,466	848,466	811,075	174,475	174,470	848,466	848,466	811,075	174,475	174,450
B: Corporations as control										
Elasticity	1.915	2.112	2.169	1.664	1.893	1.963	2.125	2.240	1.744	1.974
	(0.273)	(0.280)	(0.256)	(0.264)	(0.255)	(0.241)	(0.247)	(0.210)	(0.221)	(0.203)
Placebo	−0.222	−0.212	0.051	0.020	0.212	−0.179	−0.120	0.003	−0.094	0.071
	(0.447)	(0.485)	(0.408)	(0.430)	(0.426)	(0.202)	(0.212)	(0.177)	(0.194)	(0.185)
Observations	32,722	32,722	32,640	21,338	21,272	32,722	32,722	32,640	21,338	21,272
Controls										
Region Fixed Effects	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Tax Office Fixed Effects	No	No	Yes	No	Yes	No	No	Yes	No	Yes
Industry Fixed Effects	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Time Trend	Flexible	Flexible	Flexible	Flexible	Flexible	Linear	Linear	Linear	Linear	Linear

Notes: The table presents intensive margin elasticity estimates from Eq. (12) estimated on the period 2006–09. Standard errors are in parenthesis, which are clustered at the firm level. The treatment group comprises partnership firms, and the results in Panel A and B are from using sole proprietorships and corporations as the control group. The estimates in column (1) are from the baseline specification; columns (2)–(5) add additional control variables. I do not observe the industry and tax office for all firms, owing to which the sample for the corresponding specifications is smaller than that for the others. Columns (6)–(10) replace year fixed effects in Eq. (12) with a linear time trend. Placebo results are from the corresponding specification estimated on the period 2006–08, with 2008 assumed as the post-reform period. The estimates are income-weighted, so that the elasticity corresponds to the parameter ε_p in Eq. (10).

Table 3
Intensive margin elasticities (2009–11).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Sole proprietorships as control										
Elasticity (2009)	2.233	2.243	2.240	1.997	2.009	2.162	2.176	2.173	1.833	1.832
	(0.077)	(0.077)	(0.077)	(0.075)	(0.075)	(0.076)	(0.077)	(0.076)	(0.073)	(0.074)
Elasticity (2010)	0.205	0.251	0.231	0.253	0.275	0.120	0.159	0.142	0.142	0.146
	(0.066)	(0.067)	(0.066)	(0.079)	(0.080)	(0.062)	(0.063)	(0.062)	(0.070)	(0.072)
Elasticity (2011)	0.025	0.083	0.052	0.099	0.112	0.070	0.128	0.095	0.122	0.139
	(0.062)	(0.062)	(0.063)	(0.075)	(0.077)	(0.061)	(0.062)	(0.062)	(0.074)	(0.076)
Observations	876,317	876,317	838,773	192,776	192,771	876,317	876,317	838,773	192,776	192,771
B: Corporations as control										
Elasticity (2009)	1.855	2.074	2.149	1.603	1.931	1.855	2.074	2.149	1.603	1.931
	(0.316)	(0.322)	(0.303)	(0.310)	(0.301)	(0.316)	(0.322)	(0.303)	(0.310)	(0.301)
Elasticity (2010)	−0.482	−0.319	−0.242	−0.301	−0.075	−0.482	−0.319	−0.242	−0.301	−0.075
	(0.346)	(0.350)	(0.334)	(0.344)	(0.337)	(0.346)	(0.350)	(0.334)	(0.344)	(0.337)
Elasticity (2011)	0.352	0.582	0.699	0.591	0.818	0.352	0.582	0.699	0.591	0.818
	(0.347)	(0.368)	(0.334)	(0.346)	(0.353)	(0.347)	(0.368)	(0.334)	(0.346)	(0.353)
Observations	45,731	45,731	45,714	31,926	31,926	45,731	45,731	45,714	31,926	31,926
Controls										
Region Fixed Effects	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Tax Office Fixed Effects	No	No	Yes	No	Yes	No	No	Yes	No	Yes
Industry Fixed Effects	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Time Trend	Flexible	Flexible	Flexible	Flexible	Flexible	Linear	Linear	Linear	Linear	Linear

Notes: The table presents intensive margin elasticity estimates from Eq. (12) estimated on the period 2006–11. Standard errors are in parenthesis, which are clustered at the firm level. The treatment group comprises partnership firms, and the two control groups here are the sole proprietorships and corporations which do not experience the 2010 tax changes (see the discussion in Section 4.1). I do not observe the industry and tax office for all firms, owing to which the sample for the corresponding specifications is smaller than that for the others. Columns (6)–(10) replace year fixed effects in Eq. (12) with a linear time trend. The estimates are income-weighted, so that the elasticities correspond to the parameter ε_p in Eq. (10).

response.[29]

Using the results from the step one, I next estimate the complete counterfactual distribution of partnership earnings in the post-reform periods using the assumption Eq. (13). This distribution for the year 2009 is shown in Fig. 5A. In the final step, I strip the observed distributions of the intensive-margin responses using the elasticities estimated in the previous section. This distribution for 2009 is shown in Fig. 5B. The difference between the observed and counterfactual distribution that has been stripped of intensive-margin responses isolates the extensive margin response to the reform, which I use to estimate the elasticities reported below.

Results — Table 4 summarizes the results. Columns (2)–(4) of the table report in each row the numbers of firms in the observed and counterfactual distributions for the given post-reform period t and the income-weighted elasticity implied by them.[30] Columns (5)–(8)

---

[29] To see the lower bound interpretation, recall that the extensive margin response to the reform could potentially occur through three distinct channels: (1) reduced entry; (2) increased exit comprising firms that exit and stop filing returns; (3) increased exit comprising firm that continue filing returns but report zero earnings. The difference between the two series in Panel A of Fig. A7 captures only the third channel of the response, as both series are affected by channels (1) and (2) equally.

[30] It is important to emphasize that the elasticity for period t here represents the aggregate extensive margin response *up to* the period. In distinction, the period t elasticity reported in the other tables represents the *additional* response in the period.

A: Number of Tax Filers

B: Number of Tax Filers

C: Observed and Counterfactual Distribution

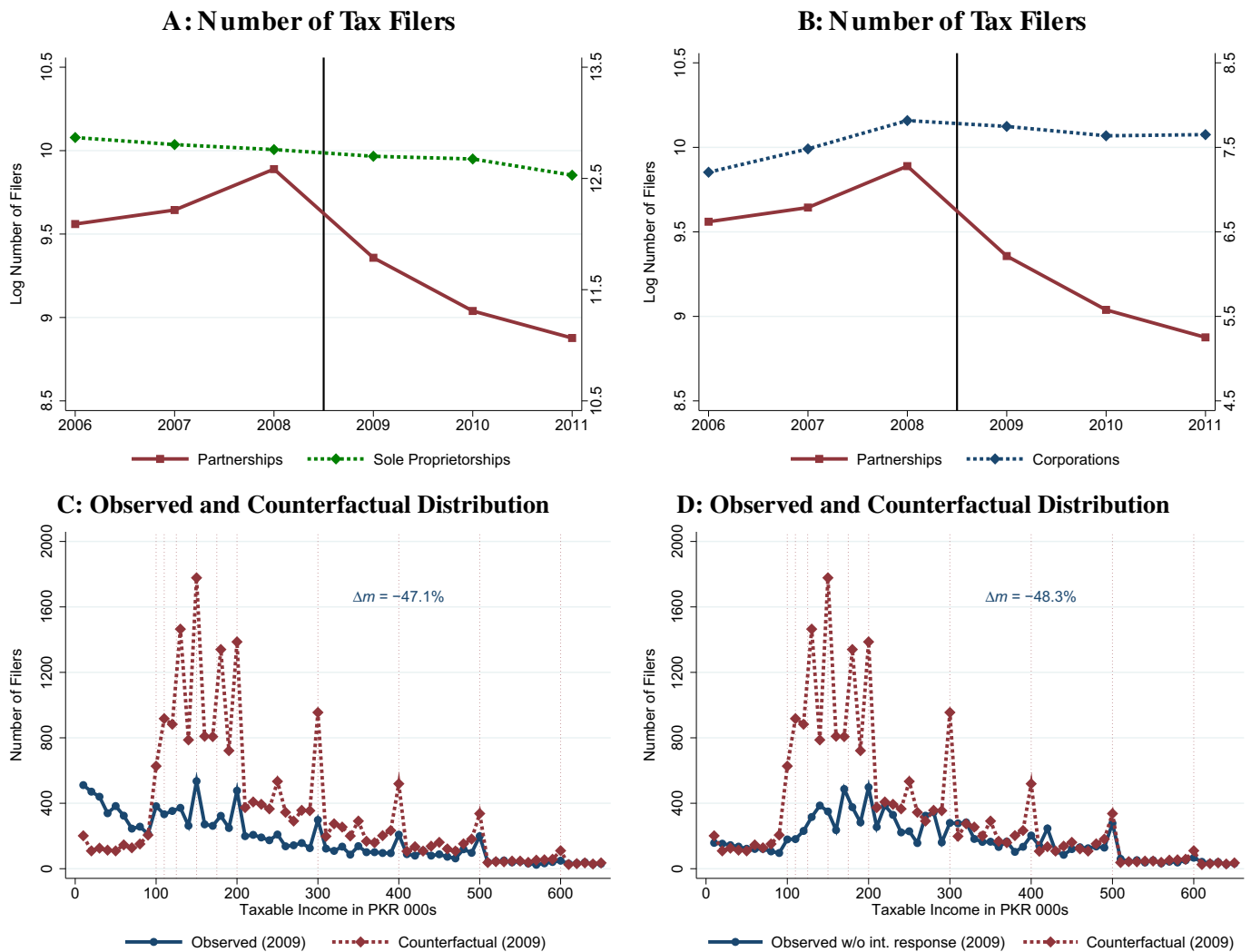## D: Observed and Counterfactual Distribution

Fig. 5. Extensive margin response. Notes: The figure depicts the strategy to estimate the extensive margin response to the reform. The top two panels compare the number of firms that report positive earnings in the range $z_{it} \in (0\ 650\ K]$ across the three groups and over time. The solid vertical line in each panel indicates the time from which the tax changes take effect. The bottom two panels illustrates the last two steps of the strategy. Panel C compares the observed and counterfactual partnership earnings distributions in 2009. The counterfactual distribution is obtained under assumption Eq. (13) by scaling up the 2008 distribution by a factor N^c_{2009}/N_{2008}, where N^c_{2009}, the counterfactual number of tax filers in 2009, is obtained from the difference-in-differences regression Eq. (12) on the two series in Fig. 5B with a linear time trend (corresponding to the specification in columns (5)–(6) of Table 4). Panel D compares the two distributions when the observed distribution has been stripped of intensive responses using the elasticities reported in Table 2. The difference in the number of firms in the two distributions as a percentage of the number of firms in the counterfactual distribution is denoted by Δm.

replicate the exercise but estimate the counterfactual number of tax filers using the two alternative specifications mentioned above. Panels A and B use sole proprietorships and corporations as controls in the first step of the strategy, whereas the estimates in Panel C are from within-partnership comparisons. Table A8, which carries the results from the auxiliary regressions Eq. (14), conducts additional robustness tests.

Three key conclusions emerge from the results. First, consistent with the visual evidence the extensive margin elasticities are large, reflecting extreme sensitivity of firms' participation choices to the tax rate. Second, in contrast to the intensive-margin response the extensive-margin response grows over time, as more firms exit and fewer firms enter the partnership sector. Third, the elasticity estimates, with the exception of ones from the baseline specification in Panel A, are reasonably robust across alternative specifications. This conclusion is strengthened further by the evidence in Appendix Tables A8 to A11, where I compare the propensity to report positive earnings across the three types of firms over time using different specifications and estimation methods. Specifically, the results are robust to *(i)* holding the composition of the sample fixed (Table A9 vs. Table A8); *(ii)* experimenting with alternative time trends (Table A9); *(iii)* conditioning on

more control variables (Table A8); and *(iv)* using a probit instead of a linear model (Table A10 vs. Table A8).

The 2010–11 elasticity estimates in Table 4 rely on the assumption that the extensive margin control outcomes were not impacted by the 2010 tax changes. This assumption, as discussed in Section 4.1, is innocuous for corporate firms. Table A11, assesses the plausibility of this assumption for the other control group. Comparing extensive margin outcomes across sole proprietorships and corporate firms, it shows that if anything the 2010 tax changes caused a slight reduction in the number of sole proprietorships with positive earnings.[31] The result, thus, reinforces the conclusion that the extensive margin elasticities from specifications that use sole proprietorships as counterfactual are underestimated.

---

[31] In fact, this should not be surprising as the most salient 2010 tax change was an increase in the exemption cutoff, which would necessarily have resulted in a decrease in the number of tax filers. Though the tax code requires all *registered* taxpayers, including those whose income is below the exemption cutoff, to file a tax return, the tax authorities are much less likely to bring action against nonfilers if their tax liability is zero.

Table 4
Extensive margin elasticities.

Year	#Obs.	#Counter.	Elasticity	#Counter.	Elasticity	#Counter.	Elasticity
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Sole proprietorships as control							
2009	11,325	16,811	0.924	19,251	1.399	23,243	1.961
			(0.026)		(0.031)		(0.044)
2010	8090	16,511	1.717	18,796	2.077	27,401	2.895
			(0.038)		(0.040)		(0.064)
2011	6955	14,817	1.743	18,352	2.311	32,301	3.338
			(0.039)		(0.043)		(0.074)
B: Corporations as control							
2009	11,325	20,150	1.545	21,923	1.798	23,243	1.961
			(0.032)		(0.039)		(0.044)
2010	8090	17,394	1.867	24,376	2.673	27,401	2.895
			(0.039)		(0.058)		(0.064)
2011	6955	17,718	2.226	27,103	3.079	32,301	3.338
			(0.041)		(0.067)		(0.074)
C: Within-partnerships comparison							
2009	11,325	20,741	1.634	–	–	–	–
			(0.034)	–	–	–	–
2010	8090	18,671	2.060	–	–	–	–
			(0.040)	–	–	–	–
2011	6955	17,388	2.179	–	–	–	–
			(0.041)	–	–	–	–
Specification		Baseline		Linear trend		Separate linear trend	

Notes: The table presents the extensive margin elasticity estimates from the three-step strategy detailed in Section 4.2. Columns (3), (5), and (7) report in each row the number of tax filers in the counterfactual distribution for the corresponding period t, whereas column (2) reports for the same period the number of tax filers in the observed distribution that has been stripped of intensive responses. The difference between the two distributions represents extensive margin response to the reform. The number of counterfactual tax filers — the first-step of the strategy — for estimates in columns (1) to (3) has been estimated using the standard difference-in-differences approach on the filers series depicted in Fig. 5. The estimates in Panel A use sole proprietorships as control, the estimates in Panel B use corporations as control, and the estimates in Panel C use partnerships with earnings in the range $z_{it} \in [0\ 650\ \mathrm{K}]$ as controls (see Fig. A7). For estimates in columns (5)–(8), I add linear and separate linear time trends to the DD specification in the first step of the strategy. The elasticities and standard errors are from regressions where the difference in the number of tax filers in the two distributions in narrow income ranges is regressed on the change in net-of-participation-tax rate experienced by taxpayers in the range. The estimates are weighted by taxable income so that the elasticities correspond to the parameter η_p in Eq. (10).

5.2.3. Income shifting

Income shifting to sole proprietorships — Fig. 6 plots the distribution of sole proprietorship earnings reported by former partnership owners, showing that the number of such owners reporting positive sole earnings went up considerably after the reform. This visual evidence on income shifting is formalized in Tables A12 and A13, where I report the results from regressions similar to Eq. (15). The results shows that there was significant income shifting towards the sole proprietorship business form: the propensity to report positive sole earnings by former partnership owners, columns (3)–(4), underwent a significant, tax-driven increase in 2009–11. But the income shifting had relatively little impact in making up for the erosion of the treated base: the tax base loss after accounting for the income shifting, columns (5)–(6), is almost as strong as one without it, columns (1)–(2). This happens because the income shifting base was so small to start with that even the relatively large proportional change (the two middle columns) resulted in a small overall effect (the difference between the first- and last-two columns). One other reflection of the modest compensatory influence of the income shifting is that the revenue-weighted elasticity $\hat{\eta}_{sp}$, estimated from Eq. (15) and reported in Table 5, is an order of magnitude smaller than the corresponding extensive margin elasticity. Table A14 performs additional specification checks on the results, highlighting their robustness to the identification concerns noted in Section 4.1.

Income shifting to corporate firms — Table A15 explores income shifting to corporations, reporting results from Eq. (15) with a dummy indicating if agent i reports positive dividend income in period t as the outcome variable. Table A16 reproduces the results on a balanced panel sample. It is important to reiterate that while income shifting to sole proprietorship could take place from 2009, owing to the fresh registration requirement (see Section 3.2) income shifting to corporations could only begin from 2010. Reassuringly, the results conform to this, reflecting significant income shifting in 2010 and 2011 but not in 2009. The magnitude of the effect, however, is extremely small: the former

partnership owners' propensity to report positive dividend income increases on average by only 0.3 percentage-points in 2010–11. This statistically significant but otherwise trivial impact is mirrored in the revenue-weighted elasticity ($\hat{\eta}_{cp}$) reported in Table 6, which is generally smaller than one-hundredth of the corresponding extensive margin elasticity.[32]

Fig. A8, which displays the result from the nonparametric permutation test (see Section 4.3), reinforces the conclusion. The estimation of Eq. (16) on daily entry data from June 6, 2009 to June 5, 2011 produces a value of -0.012 for $\hat{\delta}$ with a standard error of 0.007. This suggests that the corporate entry did not deviate significantly from the pre-existing trend at the time of the reform. To put the result into perspective, the permutation test compares it against the distribution of the placebo coefficient obtained from the equation. The estimated coefficient is right in the middle of the distribution, supporting the above result that the reform had no meaningful impact on the incorporation margin.[33]

5.2.4. Spillover effects on the VAT base

Table 7 probes the negative impact of the income tax reform on government revenue from the value-added tax (see Fig. A9 for the nonparametric evidence). I split the sample into firms subject and not

---

[32] This result should not be surprising when seen in light of the costs, both fixed and variable, that operating a firm as a corporation entails. Corporations for example are required to *(i)* register with the SECP after paying a nontrivial fee; *(ii)* register with a host of other federal, provincial, and local departments; *(iii)* maintain certified audited accounts; *(iv)* comply with labor, social security, and other related regulations; *(v)* act as tax withholding agents, deducting tax at source on transactions with other firms. For small firms, these costs could easily dwarf the productivity gains or tax savings that might accrue from incorporation.

[33] I also explore income shifting to the formal wage-earning sector, finding that only a few former partnership owners (less than 20) became wage-earners in periods following the tax reform. For all practical purposes, therefore, we can ignore this additional channel through which income shifting could occur.

A: Sole Proprietorship Income of Partnership Owners – Prereform

$\Delta m_{2008} = -1.5\%$
$\Delta m_{2007} = -3.8\%$

2006 ⬤ 2007 ◆ 2008 ▲
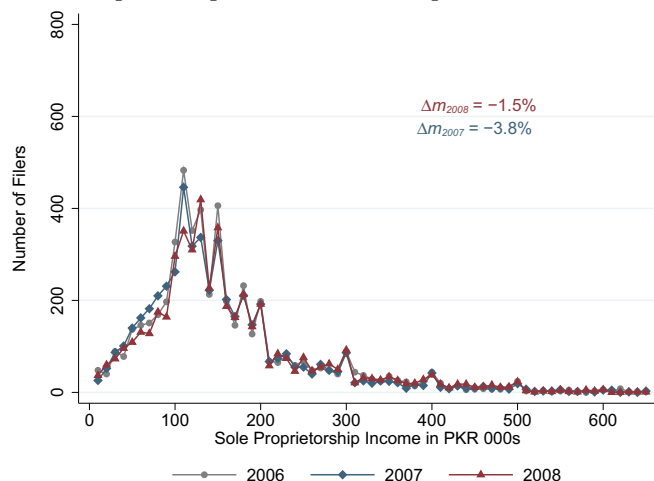
B: Sole Proprietorship Income of Partnership Owners – Post-reform

$\Delta m_{2011} = -3.9\%$
$\Delta m_{2010} = 17.2\%$
$\Delta m_{2009} = 17.9\%$

2008 ▲ 2009 ⬤ 2010 ◼ 2011 ◆

Fig. 6. Income shifting to sole proprietorships. Notes: The figure plots sole proprietorship earnings reported by former partnership owners from 2006 to 2011, exploring thereby income shifting towards the business form. Partnership owner here is defined as an individual who reports earnings from a partnership in any of the three prereform periods 2006–08. Each dot on the curves represents the upper bound of a PKR 10,000 bin and denotes the number of partnership owners who report sole earnings within that bin. While plotting the 2010–11 distributions, I partial out the intensive responses to the 2010 tax changes using Eq. (13). Yearly changes in the number of tax filers are shown by Δm_t, which for year t signifies the change in the number of filers from year $t − 1$ to t as a percentage of the number of filers in year $t − 1$.

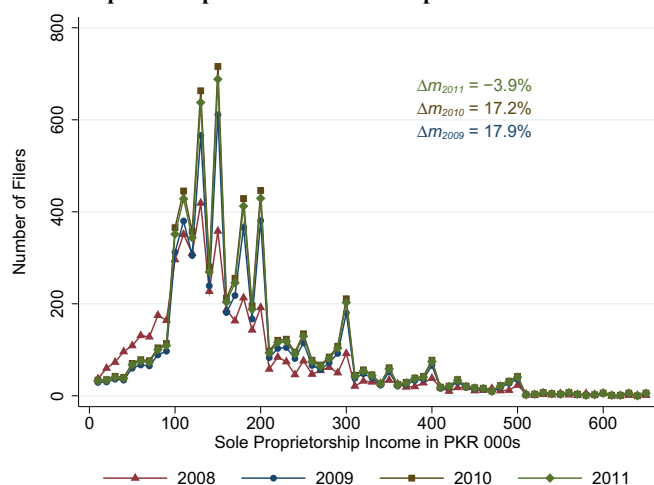subject to VAT and use Eq. (12) and the three-step strategy to estimate the intensive and extensive margin elasticities for the two groups of firms separately. The results show that although the responses of VAT-registered firms are generally smaller than those of the other firms, they nevertheless are substantial. This creates two mutually reinforcing forces – the component of the tax base not subject to VAT is more elastic and the component subject to VAT though less elastic faces a considerably higher tax rate – that will push the efficiency costs of the reform beyond those given by formula (10). In Section 6, I use the methodology described in Appendix A.2 to quantify this additional welfare impact, showing that incorporating the fiscal externality raises the costs of the reform by nearly 40%.

5.2.5. Heterogeneity

To explore response heterogeneity, I estimate the triple-difference versions of Eqs. (12) and (14), interacting the double-difference terms with the firm-characteristic variable. I study six firm-characteristics, which proxy for the size, sophistication, and transparency of operations of a firm (details in Appendix A.1). To avoid making strong functional form assumptions, the variables are introduced into the equations nonparametrically as dummies. The results, reported in Tables A17 and A19, show that firms with any of the six characteristics respond considerably less aggressively relative to the other firms. Relatedly, Table A18 and Fig. A10 evaluate if the responses vary across income groups or if it is easier for firms in a few industries to become informal.[34] Overall, the results are broadly in line with the recent theoretical literature (see for example Gordon and Li, 2009, Kleven et al., 2016) that emphasizes the importance of information environment – the extent to which the income generation process in a firm leaves verifiable information trails for the government – as the key determinant of firm compliance.

5.3. Discussion

Real vs. evasion margins — Can governments curtail efficiency costs arising from behavioral responses to taxation? The answer to the question depends on the nature of the responses. Real responses are a function of deep structural parameters of technology and preferences, and therefore are not particularly amenable to policy intervention. In contrast, governments can always curb evasion and avoidance by improving tax design or investing in the enforcement capacity. The estimates in Table 3 show that the *additional* earnings response in 2010–11 was negligible relative to the 2009 response. This under unanticipated retroactive application implies that the predominant channel through which firms responded to the tax increase was tax evasion. This finding is further supported by looking at the evolution of important line items reported on the tax return form. Fig. A11 carries out this exercise, showing that the line items more tightly linked to the real side of business activity, such as profit and loss expenses and inventories (the bottom two panels), do not respond at all in 2009. No change in these line items – especially in profit and loss expenses which include input costs such as wages, rents, utility payments, and thus are more likely to be third-party reported – compared to the large decrease in taxable income (Fig. 4), sales (Fig. A11), and cost of sales (Fig. A11) supports the tax evasion interpretation of the observed 2009 response.

Unlike the intensive response, I am less certain about the exact nature of the extensive margin response. Table 4 shows that the tax rate increase led to fewer and fewer formal firms over time. While some of the missing firms would have migrated to the informal sector, the others would have shut down completely. Unfortunately, the two extensive margin choices that I observe in the data – firms reporting zero earnings or disappearing completely after the reform – are potentially consistent with both explanations. But, as noted earlier, considering the lack of public support available to the exited taxpayers through social insurance, it would be natural to consider that the extensive response in large part reflects exit into informality.

External validity — Though the empirical focus of this paper are partnerships, for at least two reasons the results are broadly representative of firm behavior to taxation in a low enforcement-capacity setting. First, in terms of their reaction to taxes partnerships are quite similar to the other types of firms. Table A20 establishes this formally by comparing the behavior of sole proprietorships and partnerships to the common baseline tax system in 2006–08. Furthermore, in terms of

---

[34] The analysis shows that the extensive response to the reform was generally homogeneous across industries with the exception of a few outliers. These outlier industries were Retail Sales; Manufacture of plastic, concrete, and plaster products; Repair of Machinery, Equipment, and Vehicles (stronger than usual response) and Real Estate Services, Accommodation Services, and Construction of Buildings (no negative response at all).

Table 5
Income shifting to sole proprietorships.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Sole proprietorships as control										
Elasticity (2009)	−0.211	−0.212	−0.212	−0.146	−0.154	−0.188	−0.201	−0.198	−0.142	−0.156
	(0.006)	(0.006)	(0.006)	(0.011)	(0.011)	(0.005)	(0.005)	(0.005)	(0.010)	(0.010)
Elasticity (2010)	−0.255	−0.261	−0.257	−0.185	−0.200	−0.280	−0.287	−0.282	−0.216	−0.233
	(0.004)	(0.004)	(0.004)	(0.008)	(0.008)	(0.005)	(0.004)	(0.004)	(0.009)	(0.008)
Elasticity (2011)	−0.323	−0.325	−0.321	−0.235	−0.250	−0.306	−0.304	−0.303	−0.233	−0.244
	(0.004)	(0.004)	(0.004)	(0.008)	(0.008)	(0.004)	(0.004)	(0.004)	(0.008)	(0.008)
Observations	2,991,995	2,991,995	2,818,285	867,567	867,551	2,991,995	2,991,995	2,818,285	867,567	867,551
B: Corporations as control										
Elasticity (2009)	−0.143	−0.147	−0.149	−0.142	−0.142	−0.137	−0.139	−0.143	−0.138	−0.138
	(0.008)	(0.008)	(0.008)	(0.011)	(0.011)	(0.006)	(0.006)	(0.006)	(0.010)	(0.010)
Elasticity (2010)	−0.267	−0.282	−0.270	−0.262	−0.277	−0.230	−0.227	−0.233	−0.229	−0.229
	(0.008)	(0.008)	(0.008)	(0.010)	(0.011)	(0.006)	(0.006)	(0.006)	(0.009)	(0.009)
Elasticity (2011)	−0.237	−0.229	−0.238	−0.238	−0.233	−0.249	−0.243	−0.251	−0.253	−0.251
	(0.007)	(0.007)	(0.007)	(0.010)	(0.010)	(0.007)	(0.007)	(0.006)	(0.009)	(0.009)
Observations	192,604	192,604	192,285	110,372	110,372	192,604	192,604	192,285	110,372	110,372
Controls										
Region Fixed Effects	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Tax Office Fixed Effects	No	No	Yes	No	Yes	No	No	Yes	No	Yes
Industry Fixed Effects	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Time Trend	Flexible	Flexible	Flexible	Flexible	Flexible	Linear	Linear	Linear	Linear	Linear

Notes: The table presents the income shifting elasticity estimates from Eq. (15), estimated on the period 2006–11. Standard errors are in parenthesis, which are clustered at the firm level. The treatment group comprises partnership owners, defined as individuals who report earnings from a partnership in any of the three prereform periods 2006–08. The outcome variable is an indicator $1(z_{j,it} > 0)$ denoting that i reports positive sole proprietorship earning (taxable income for corporations) in period t. I do not observe the industry and tax office for all firms, owing to which the sample for the corresponding specifications is smaller than that for the others. The estimates are revenue-weighted so that the elasticities correspond to the parameter $\hat{\eta}_{sp}$ in Eq. (10).

Table 6
Income shifting to corporations.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Sole proprietorships as control										
Elasticity (2010)	−0.013	−0.011	−0.011	−0.023	−0.016	−0.012	−0.010	−0.010	−0.019	−0.012
	(0.003)	(0.003)	(0.003)	(0.007)	(0.007)	(0.004)	(0.003)	(0.003)	(0.007)	(0.007)
Elasticity (2011)	−0.015	−0.013	−0.013	−0.031	−0.024	−0.015	−0.015	−0.015	−0.029	−0.023
	(0.004)	(0.004)	(0.004)	(0.007)	(0.007)	(0.004)	(0.004)	(0.004)	(0.007)	(0.007)
Observations	2,991,995	2,991,995	2,818,285	867,567	867,551	2,991,995	2,991,995	2,818,285	867,567	867,551
B: Corporations as control										
Elasticity (2011)	−0.022	−0.027	−0.024	−0.021	−0.025	−0.018	−0.022	−0.020	−0.020	−0.022
	(0.004)	(0.005)	(0.004)	(0.008)	(0.008)	(0.004)	(0.004)	(0.004)	(0.008)	(0.008)
Elasticity (2010)	−0.026	−0.035	−0.029	−0.031	−0.035	−0.024	−0.035	−0.029	−0.033	−0.039
	(0.004)	(0.005)	(0.005)	(0.008)	(0.009)	(0.005)	(0.005)	(0.005)	(0.009)	(0.009)
Observations	192,595	192,595	192,276	110,363	110,363	192,595	192,595	192,276	110,363	110,363
Controls										
Region Fixed Effects	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Tax Office Fixed Effects	No	No	Yes	No	Yes	No	No	Yes	No	Yes
Industry Fixed Effects	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Time Trend	Flexible	Flexible	Flexible	Flexible	Flexible	Linear	Linear	Linear	Linear	Linear

Notes: The table presents the income shifting elasticity estimates from Eq. (15), estimated on the period 2006–11. Standard errors are in parenthesis, which are clustered at the firm level. The treatment group comprises partnership owners, defined as individuals who report earnings from a partnership in any of the three prereform periods 2006–08. The outcome variable is an indicator $1(z_{j,it} > 0)$ denoting that i reports positive dividend income in period t. I do not observe the industry and tax office for all firms, owing to which the sample for the corresponding specifications is smaller than that for the others. Columns (6)–(10) replace year fixed effects in Eq. (12) with a linear time trend. The estimates are revenue-weighted so that the elasticities correspond to the parameter $\hat{\eta}_{cp}$ in Eq. (10).

Table 7
Spillover effects on the VAT base.

	Firms subject to VAT			Firms not subject to VAT		
	(1)	(2)	(3)	(4)	(5)	(6)
A: Sole proprietorships as control						
1. Intensive Margin:						
Elasticity	1.424	1.364	1.254	2.681	2.676	2.690
	(0.130)	(0.133)	(0.136)	(0.096)	(0.099)	(0.098)
Observations	51,556	51,556	7074	796,910	796,910	22,417
2. Extensive Margin:						
Elasticity	0.677	1.499	2.569	2.121	2.626	3.556
	(0.033)	(0.034)	(0.071)	(0.045)	(0.048)	(0.073)
Observations	51,556	51,556	7074	796,910	796,910	22,417
B: Corporations as control						
1. Intensive Margin:						
Elasticity	1.241	1.102	1.249	2.238	2.400	2.690
	(0.496)	(0.425)	(0.136)	(0.288)	(0.240)	(0.098)
Observations	8203	8203	7073	24,519	24,519	22,417
2. Extensive Margin:						
Elasticity	0.363	1.974	2.490	2.787	3.430	3.551
	(0.041)	(0.051)	(0.068)	(0.053)	(0.070)	(0.073)
Observations	8203	8203	7074	24,519	24,519	22,417
Specification	DD	DD	TS	DD	DD	TS
Time Trend	Flexible	Linear	Flexible	Flexible	Linear	Flexible

Notes: The table replicates the analysis in Tables 2 and 4, stratifying the sample by VAT registration of firms. The intensive margin estimates in columns (1)–(2) and (5)–(6) are based on Eq. (12), and correspond to the estimates in columns (1) and (6) of Table 2. The extensive margin estimates in columns (1)–(2) and (5)–(6) are from the three-step strategy, and correspond to the estimates in columns (4) and (6) of Table 4. The estimates in column (3) and (6) are the time series counterparts, and correspond to the related estimates in columns (1) of Table A6 and (8) of Table 4. The standard errors are in parenthesis, which are clustered at the firm level. All estimates are income-weighted, so that the elasticities correspond to the parameters ε_p and η_p in Eq. (10).

their compliance attributes partnerships lie in between the other two types of firms (Table 1), making their behavior typical of an *average* firm in the country. Second, partnerships in Pakistan are not limited to human-capital intensive industries such as accounting and law but are represented fairly in all industries (Fig. A1).[35]

Relatedly, it is commonly known in literature that earnings responses to a tax reform depend on its design, in particular the income shifting opportunities it affords (see for example Slemrod and Kopczuk, 2002). The reform exploited here was targeted to a narrow section of the tax base, thus creating significant opportunities for income shifting. This, however, does not diminish the external validity of the results, as I am able to identify income shifting cleanly. Once these fiscal externalities are netted out, the leftover responses characterize what would occur if a similar but broad-based tax increase is implemented.

6. Welfare analysis

In this section, I use formula (10) to compute the welfare costs of the reform. To express these costs in more intuitive terms, I compute the following statistic

$$-\frac{dW/d\tau_p}{dW/d\tau_p|_{z_p}} = \frac{\tau_p(\varepsilon_p + \eta_p + \widehat{\eta}_{sp} + \widehat{\eta}_{cp})}{1 - \tau_p}, \tag{17}$$

where $\frac{dW}{d\tau}\big|_{z_p}$ is the change in welfare absent any behavioral response, commonly known in the literature as the mechanical effect of a tax change. The statistic expresses the behavioral revenue loss caused by the reform as a share of its mechanical effect. If this share is larger than one, the government would lose more revenue than the maximum it

could have gained from the tax increase, implying that the new tax rate was not optimal. Using the most conservative estimates of the four elasticities estimated in the last section, I obtain a value of 1.25 for the statistic in 2011. This illustrates that the new, flat tax rate of 25% on partnership earnings was on the wrong side of the Laffer curve and would not have been optimal under any social preferences.[36] This computation ignores the negative externality operating through the reduction in VAT revenue. Using the methodology developed in Appendix A.2 and the elasticities reported in Table 7, I find that the value of the statistic rises by more than 40% once the additional impact is taken into account.

An alternative, and perhaps more transparent, way to show that the new rate of 25% was above the Laffer bound is to compare the predicted post-reform revenue under the baseline tax system to the actual revenue realized after the reform. The counterfactual partnership earnings distributions estimated in Section 5.2.2 (see Fig. 5) allow me to predict the revenue the government would have obtained in 2009–11 had it not changed the tax system. I find that the predicted revenue is strictly larger than the actual revenue realized in 2011, showing that the tax rate increase led to a decrease rather than an increase in revenue. I provide the details of the methodology used for this exercise in Appendix A.3.

7. Conclusions

Firm behavior to taxation in settings characterized by low enforcement capacity and large informality has been understudied, primarily because the relevant data were not available till recently. This paper uses the population of income tax returns filed between 2006 and 2011 in Pakistan to show how firms engage in tax evasion, migrate into informality, and switch business organization to counter an increase in their tax burden. Elasticities underlying these responses are an order of magnitude larger than ones estimated in rich countries, highlighting the small costs at which firms in developing countries are able to manipulate their earnings following a tax increase.

The identifying variation in the paper comes from a policy reform introduced in 2009. The Pakistani context offers three key advantages. First, the reform creates tax rate variation across very similar firms, thereby producing almost ideal comparison groups to disentangle macroeconomic shocks from the tax-driven responses. Second, the reform was given retroactive effect that helps decomposing the observed responses into real and evasion margins. Third, the context, in particular the richness of the data, permits a clean identification of key fiscal externalities – income shifting and negative impact on the VAT base – arising from the change in marginal income tax rate. Taking the spillovers into account leads to more robust measurement of welfare loss arising from the tax change.

The results underscore that informality and tax evasion remain the first-order challenges stifling the development of fiscal capacity in emerging economies. Following the tax increase, the number of formal, treated firms fell sharply (elasticity ≈ 3), and surviving firms' earnings registered a steep decline (elasticity ≈ 2). Though, some of the losses were offset by income shifting within the formal sector, the size of the countervailing effect was extremely modest. Accordingly, the overriding conclusion that one draws from the results is that unless the costs of noncompliance are increased markedly, through investment in enforcement capacity or a change in tax design, raising taxes in emerging economies would continue to entail crippling economic costs.

---

[35] A more or less similar trend is observed in the US. See Cooper et al. (2015) for the distribution of tax-paying firms in the US stratified by their organizational form.

[36] This conclusion remains unaltered even if we take into account the marginal efficiency gains accruing from the replacement of the notch-based, preform tax schedule with the flat, post-reform tax schedule.

Appendix A

A.1. Details of firms characteristics

 (i) **Firm size.** The dummy variable takes the value 1 for firms with sales above the 75th percentile of the size distribution,[37] where firm size is defined as the average annual sales in the three prereform periods.
 (ii) **Electronic return filer.** All partnerships were required to file electronic returns in years 2008–11. Some of the firms did not comply with the mandatory provision, while a few were filing electronically even before the mandate came into effect. I categorize a firm electronic filer if any of the four returns for tax years 2006–09 was filed electronically (about 80% of the firms).
(iii) **Registered for VAT.** The variable indicates if the firm was registered with the FBR to remit VAT on its sales.
(iv) **Firm age.** The dummy variable takes the value 1 if age of the firm – measured in the number of years since registering with the FBR – was more than the 75th percentile (6 years).
 (v) **Tax withholding.** Pakistani tax code stipulates a very elaborate tax withholding scheme. In addition to wages, tax is withheld on a number of other transactions including the payment for goods and services, utility bills, cash withdrawal from banks, and imports from other countries. The withheld tax can be adjusted against the tax liability at the time of filing of returns. The firms which withhold tax are required to file a statement with the FBR indicating the transactions and the tax withheld thereon. The scheme has some elements of third party reporting, though it does not provide information on the total tax base as is the case with tax withholding on wages. The dummy variable takes the value 1 if the withheld tax of a firm weighted by its taxable income was more than the 75th percentile in the preform periods.
(vi) **Withholding agent.** The variable is an indicator if the firm was a withholding agent, required by the tax code to withhold tax on transactions made with its buyers or sellers (about 22% of the firms).

A.2. Spillover effect on the VAT base

Formula (10) ignores one important feature of the tax environment that a subset of firms also remit VAT on their sales. The income-tax-driven changes in firm behavior will impact government revenue from the VAT base as well, increasing the costs of the reform above those given by the formula. To incorporate this fiscal externality into the welfare calculations, note that the government revenue in this general setting is given by

$$T(\tau, \theta) = \sum_j \left[\int_{\theta \in M_j^v(\tau)} (\tau_j + \tau_j^v) z_j(\tau, \theta) dF(\theta) + \int_{\theta \in M_j^{v'}(\tau)} \tau_j z_j(\tau, \theta) dF(\theta) \right],$$

(A1)

where $M_j^v(\tau)$ and $M_j^{v'}(\tau)$ are the two mutually exclusive subsets of $M_j(\tau)$ consisting of firms of type j which are subject and not subject to VAT, z_j are reported earnings, and τ_j^v is the effective VAT rate on these earnings. The above expression shows that as long as the tax change $d\tau_p$ does not cause a movement of firms from $M_j^v(.)$ to $M_j^{v'}(.)$ its welfare impact can be computed simply as the weighted sum of the impact on the two constituent bases.[38] Note that this is a very general formulation that allows the two types of firms to have different elasticities, which is important because VAT-registered firms are linked to their supplies and buyers through the invoice-credit mechanism and thus might have lower ability to manipulate their earnings after a tax change. In the empirical application, I therefore compute the welfare costs of the reform in two iterations. I first use formula (10), ignoring thereby the negative VAT externality. I then take the externality into account, and use the elasticities from Table 7 to compute the aggregate welfare loss as a weighted average of the loss in the bases subject and not subject to VAT.

A.3. Was the post-reform tax rate above the Laffer bound ?

In Section 6, I use formula (17) to show that the new tax rate of 25% was on the wrong side of the Laffer curve. In this section, I derive the result using a more intuitive and transparent method.

In Section 5.2.2, I construct the counterfactual distributions of partnership earnings in the post-reform periods. Using the distributions, I predict the counterfactual revenue that would have been realized in a given post-reform year had the tax system remained unchanged. Given that the counterfactual distributions are estimated in bins of PKR 10 K, I use the approximation that the average income within a bin is the mid-point of the bin. For example, I treat reported income of all firms in the (0 10,000] bin as PKR 5000. Multiplying the average income with the number of firms and the baseline tax rate gives me the counterfactual revenue in a particular bin. I then aggregate the revenue from the entire binned distribution for the year. This counterfactual revenue for years 2009–11 is shown in the second column of the following table. I compare it to the revenue actually realized in these years in the third column. Columns (4)–(5) make similar comparisons but also take into account VAT remitted by the firms.[39] The behavioral revenue loss caused by increasing the tax rate to 25% was so large that by the third year (second year if we take VAT into account) after the reform the government was collecting less revenue than it would have under the baseline tax rates. This clearly shows that the new tax rate was set above the Laffer bound.[40]

---

[37] The cutoff choice reflects the strongly skewed firm size distribution. The 75th percentile corresponds to an annual turnover of Rs. 6.6 million (US $ 62,857). Compared with this the median firm has a turnover of Rs. 1.9 million (US$ 17,749) only.

[38] In Pakistan, firms subject to VAT are separately registered and are not allowed to drop out of the VAT net unless their sales, verified through a process of detailed audit, fall below the exemption cutoff (Rs. 5 million). For this reason, de-registration from VAT is generally a very costly process and the assumption that firms do not move across the two sets is a good description of the empirical setting.

[39] To compute the VAT revenue, I make the very conservative assumption that the value-added of a firm – sales minus the cost of raw materials – is twice its taxable income.

[40] Note that it is quite a conservative assessment, as in addition to the income tax and value-added tax the government would also lose other small federal, provincial, and local taxes that are recovered from registered firms.

Table A1
The counterfactual and realized revenue.

	Without VAT			With VAT	
Year	Counterfactual	Realized		Counterfactual	Realized
(1)	(2)	(3)		(4)	(5)
2009	308	581		723	799
2010	363	465		852	648
2011	428	409		1005	573

All figures are in PKR millions.
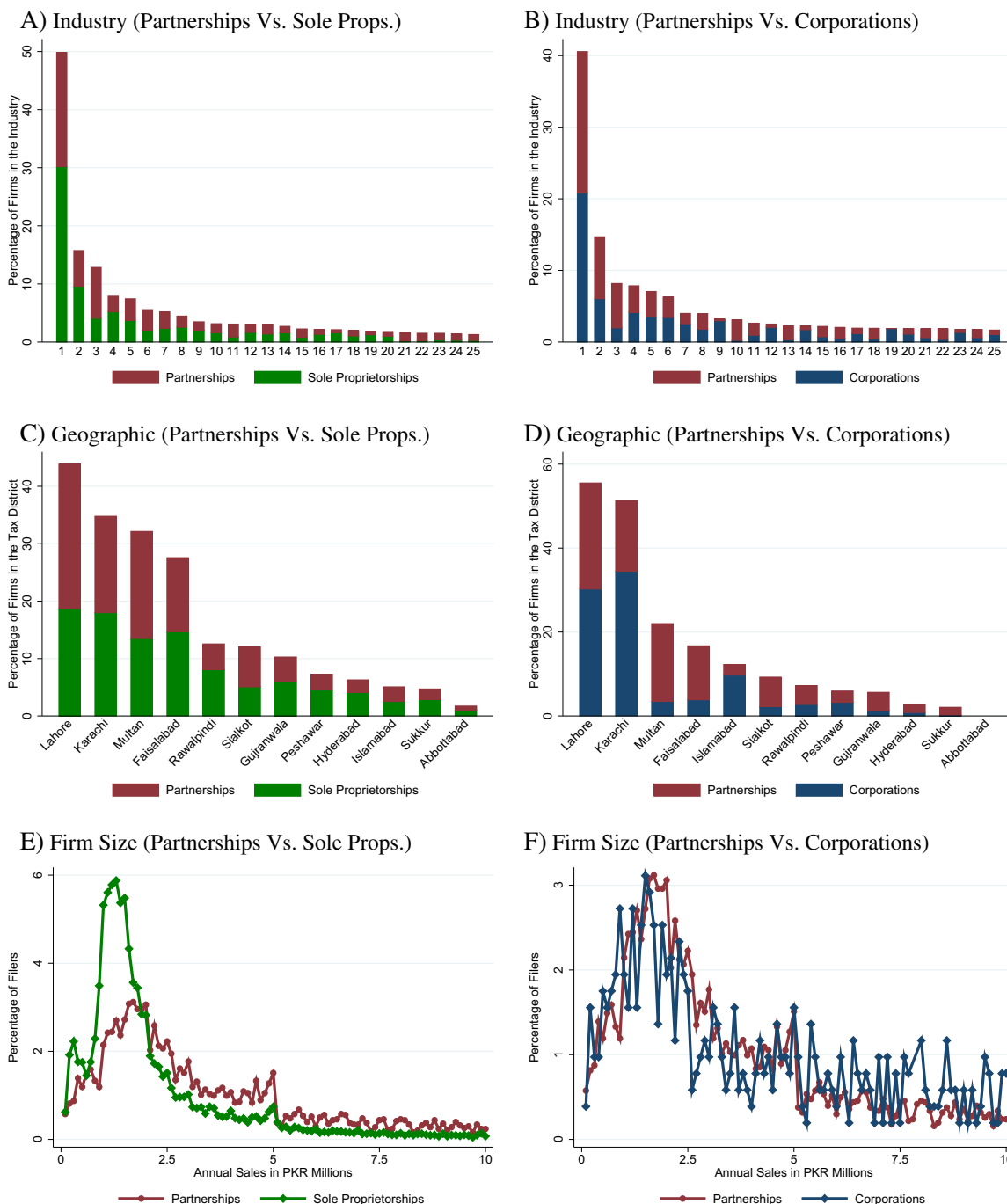
Fig. A1. Industry, geographic, and size distribution of firms. Notes: The figure shows the industry, geographic, and size distribution of firms in the baseline year 2008. The top two panels compare the distribution of firms across the top 25 industries in Pakistan. The detailed description of these 2-digit industries is given in Table A21. The number on the x-axis corresponds to the industry label (column 1) in the table. The middle panels compare the distribution of firms across major cities in Pakistan. The lengths of blue and red bars in panels A–D show the proportion of each type of firm in the particular industry or city. For example Industry 1 contains around 30% of all sole proprietorships and around 20% of all partnerships in Pakistan. The bottom two panels compare the distribution of firm-size across the three types of firms. Each dot in the plots represents the upper bound of a PKR 100 K bin and denotes the percentage of firms with annual sales within that bin.
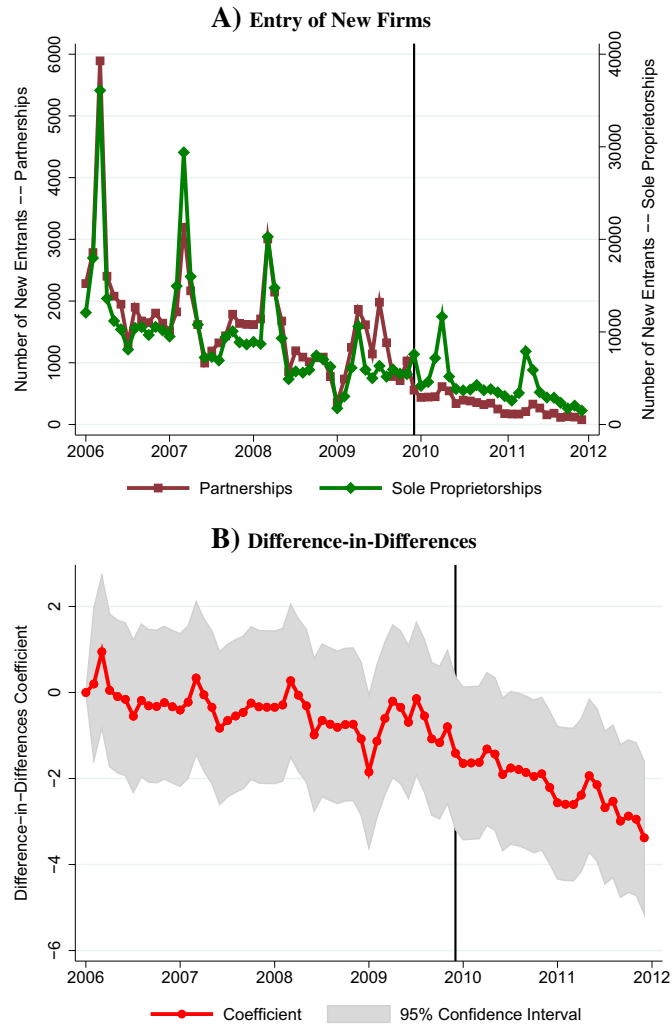
Fig. A2. Was the reform anticipated? Notes: The figure replicates the analysis in Fig. 2, using sole proprietorships as controls. For sole proprietorships, I observe the date of registration only if the firm files a return, as they are not required to register separately from their owners. The analysis in this figure is, accordingly, limited to the subset of firms which file tax return in the sample period. For this reason, the two entry series decline mechanically over time and are more noisy. The results nonetheless are consistent with those in Fig. 2 which use corporate firms as control.
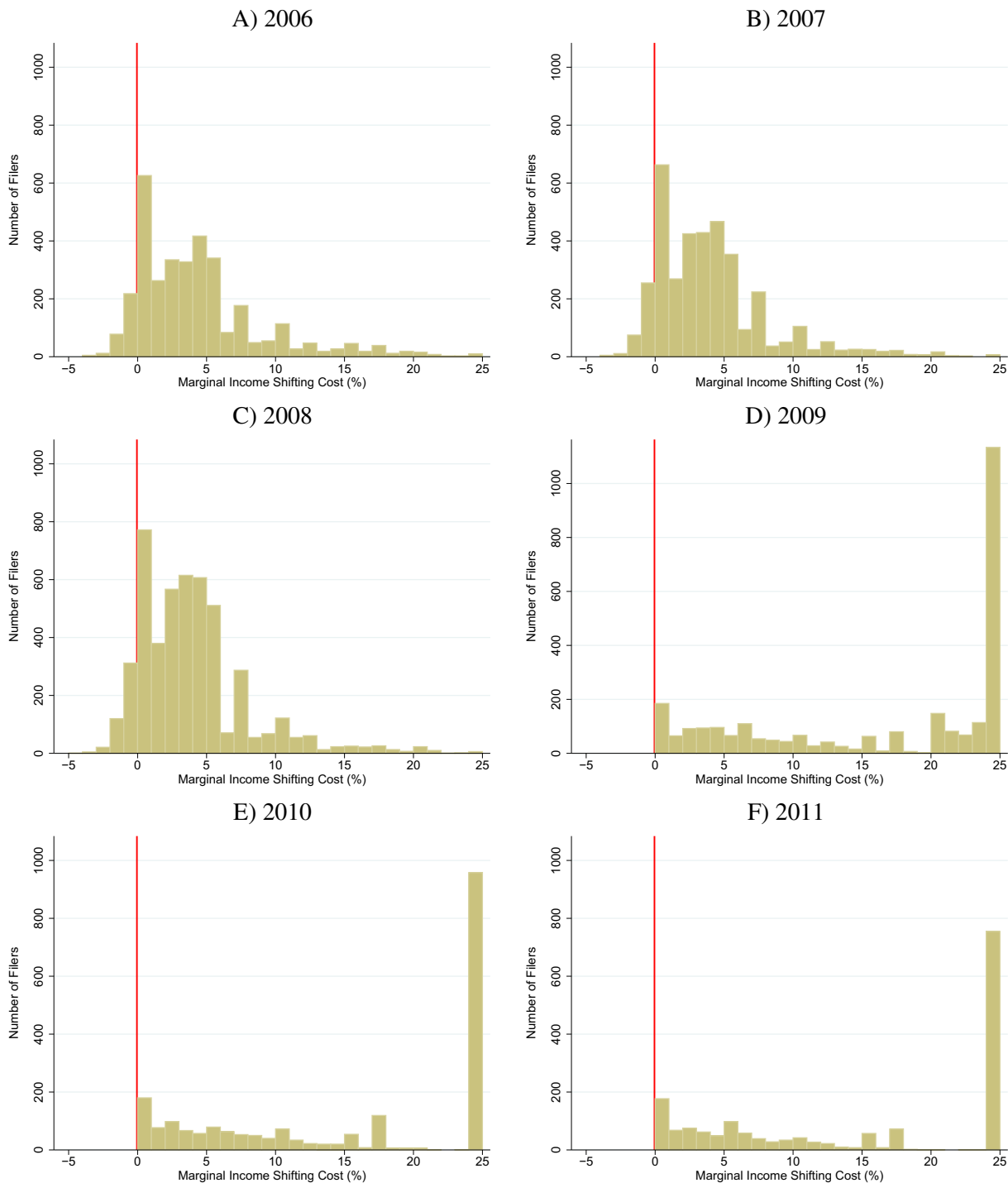
Fig. A3. Distribution of income shifting costs. Notes: The figure depicts the distribution of income shifting costs. The variable on the x-axis captures the minimum profit gain a firm experiences from operating as a partnership rather than a sole proprietorship. If the firm changes its business organization after the tax increase, it will lose at least this much of profits. The variable, thus, represents a lower bound on the income shifting costs. The details on how these costs are calculated are in Section 3.5. The distributions are shown in bins of size 0.83, where each bin includes the upper bound of the interval. The solid vertical line demarcates the boundary below which such costs are negative. It is important to emphasize that the three post-reform panels are not directly comparable to the three prereform panels, as some partnerships drop out of the sample in 2009–11.
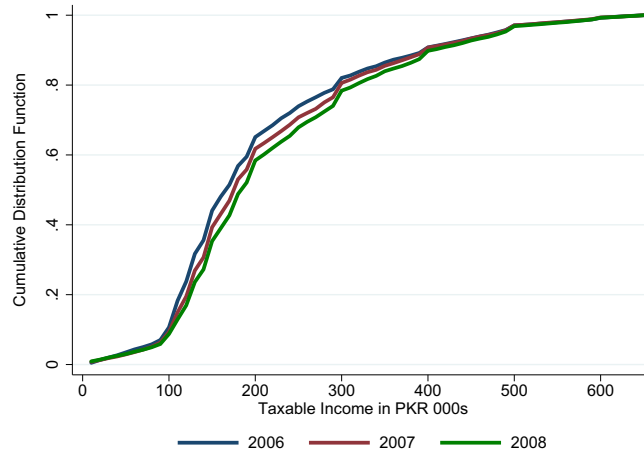
Fig. A4. Empirical earnings distribution function. Notes: The figure plots the Cumulative Distribution Function (CDF) of partnership earnings in Pakistan for the three preform periods 2006–08. The three curves of the figures plot for each year t the probability $Pr[z_{p,it} \le x]$ as the cutoff x is varied from 0 to 650 K in intervals of 10 K. That these curves are almost on top of each other provides a direct test in support of Eq. (13).
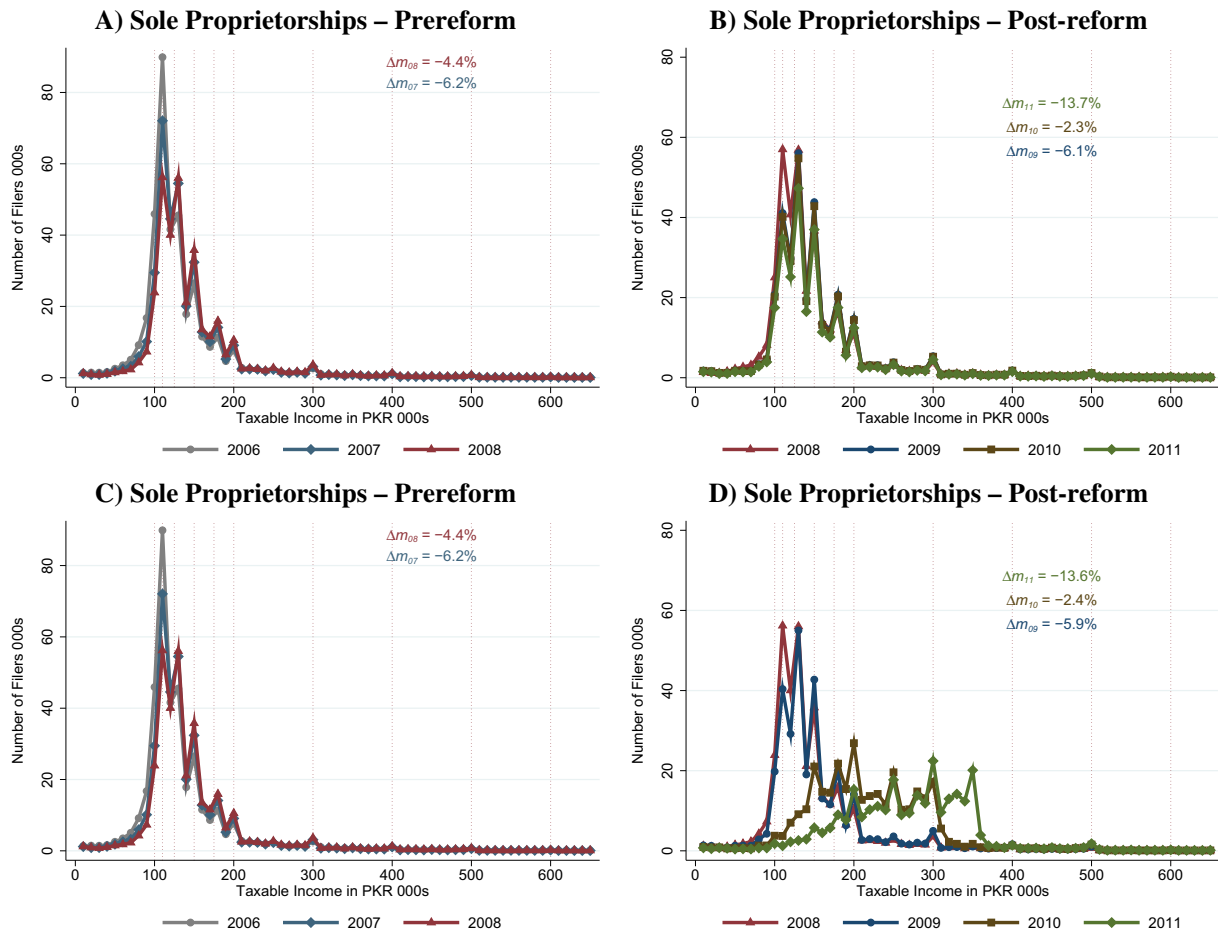
Fig. A5. Taxable income distribution. Notes: While plotting sole proprietorship earnings distribution in Fig. 3C–D, I (i) drop sole proprietors that report any income from a partnership in 2006–11 and (ii) strip the 2010–11 distributions of intensive responses to the 2010 tax changes using Eq. (13). This figure depicts the sole proprietorship earnings distribution without making these changes. The top panels replicate panels C and D of Fig. 3 without making the change (i), and the bottom panels replicate panels C and D of Fig. 3 without making the change (ii). Each dot on the curves represents the upper bound of a PKR 10,000 bin and denotes the number of firms which report earnings within that bin. The notches in the 2006–08 schedule are shown by the vertical dotted lines. In the right-hand side panels, the 2008 distribution is shown again for comparison purposes. Yearly changes in the number of tax filers are shown by Δm_t, which for year t signifies the change in the number of filers from year $t-1$ to t as a percentage of the number of filers in year $t-1$.
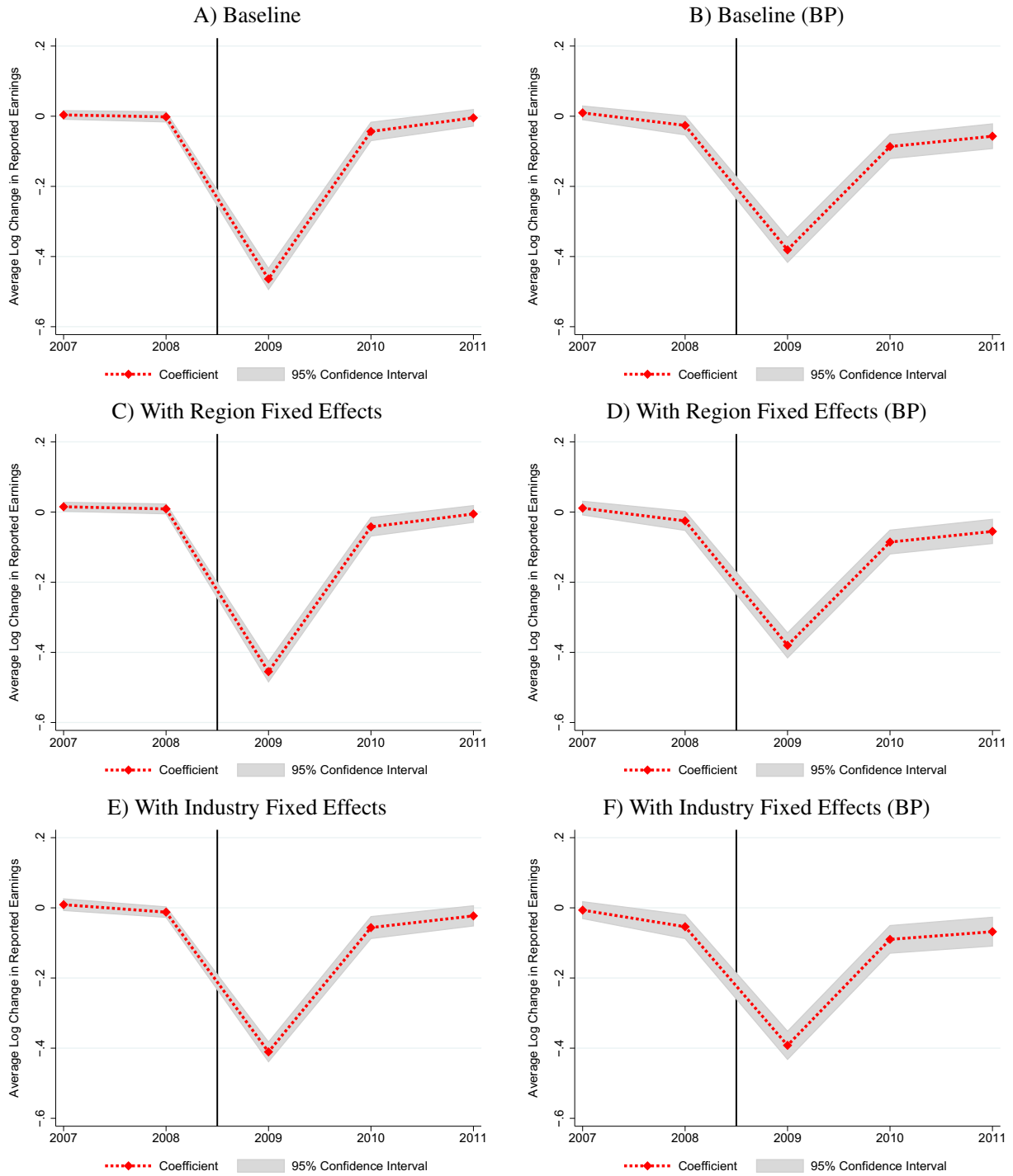
Fig. A6. Robustness of intensive margin estimates. Notes: The figure portrays the robustness of the intensive margin estimates. Each panel of the figure illustrates the results from the following regressions similar to Eq. (12)

$$\Delta \log z_{it} = \alpha + \textbf{Partnership} \times \textbf{Year}_{it}\boldsymbol{\beta} + X_i\,\boldsymbol{\delta} + \lambda_t + u_{it},$$

where **Partnership** × **Year** is a vector of three interaction dummies one for each year 2007 to 2011. Each panel of the figure plots the coefficients and the corresponding 95% confidence intervals on the five interaction dummies. Note that I have to drop the partnership dummy in order to show the double-interaction coefficients for the five periods, so the results though very similar are not directly comparable to those in Table 3. The LHS panels include all firms, whereas the RHS panels show the results for the same specification estimated on the balanced panel sample. The sample for the estimation include both partnerships and sole proprietorships, and all estimates are weighted by taxable income. The baseline specifications in panels A–B do not include any control variables. The specification in panels C–H include region, industry, and both region and industry fixed effects. The specifications in panels I–L replace the year fixed effects with the linear and industry-specific time trends. The industry-specific time trend specification includes industry, year, and industry × year fixed effects.

G) With Region and Industry Fixed Effects

H) With Region and Industry Fixed Effects (BP)

I) Linear Trend

J) Linear Trend (BP)

K) Industry-specific Trend

L) Industry-specificTrend (BP)
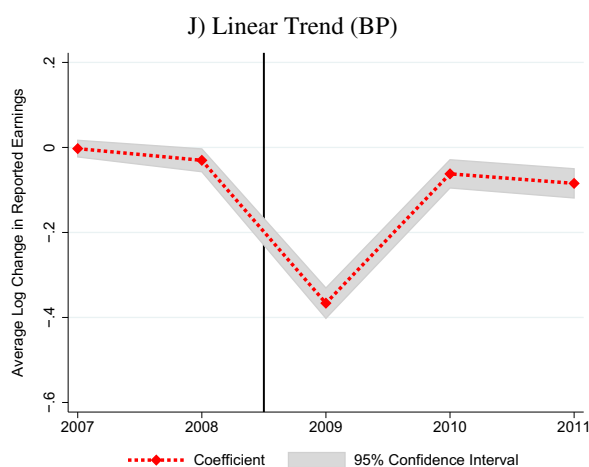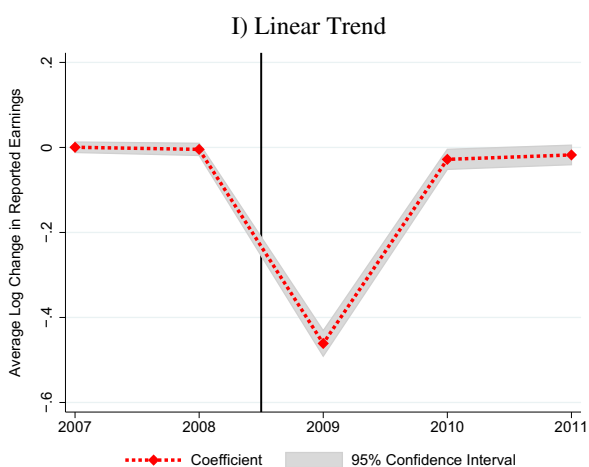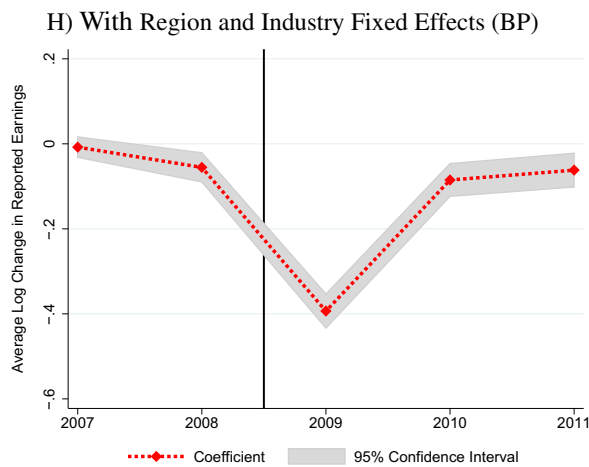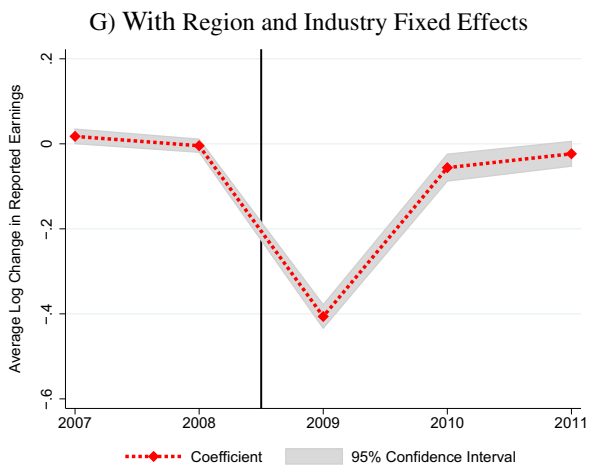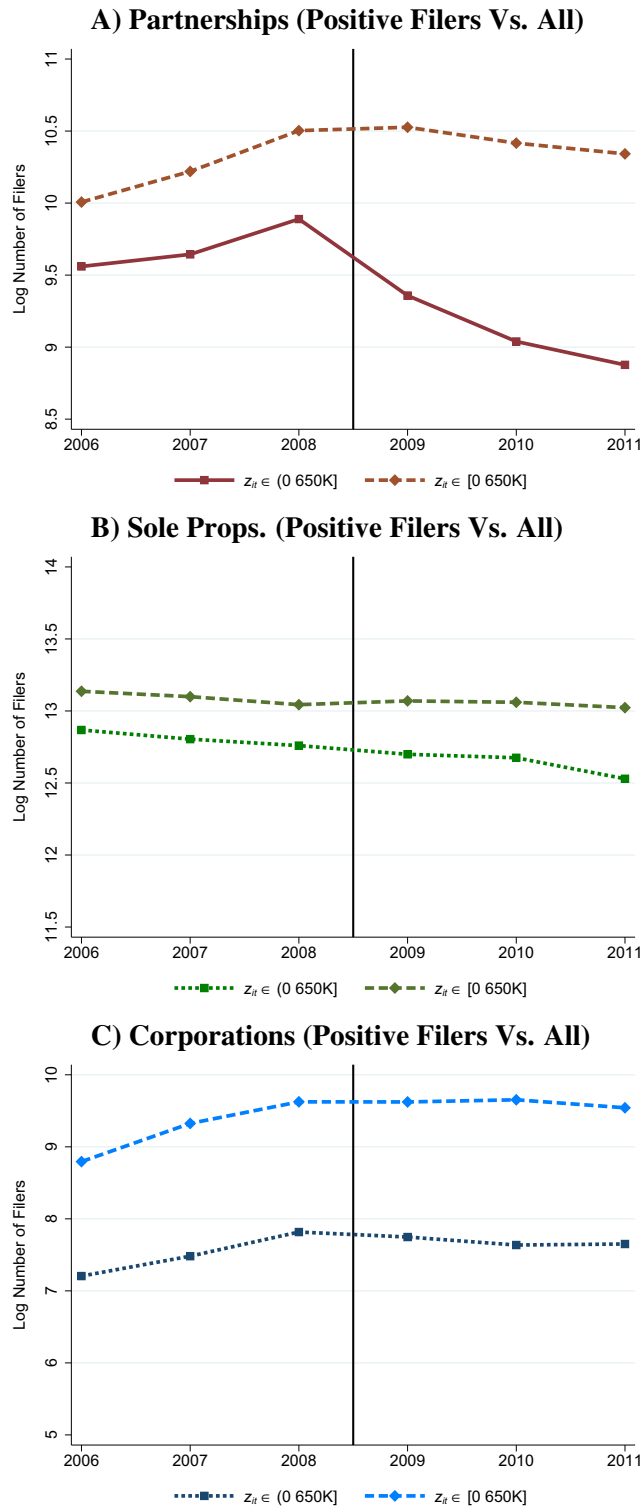
Fig. A7. Extensive margin response. Notes: The figure depicts the extensive margin response to the reform. The three panels compare the number of firms that report earnings in the range $z_{it} \in (0\ 650\ \text{K}]$ to the number of firms that report earnings in the range $z_{it} \in [0\ 650\ \text{K}]$ across the three types of firms. The difference between the two series in each panel captures firms that report zero earnings in a given year. For treated firms, the difference was remarkably stable in the prereform years but grew sharply afterwards. For control firms, the difference remained stable through out the period 2006–11. The solid vertical line in each panel indicates the time from which the tax changes take effect.
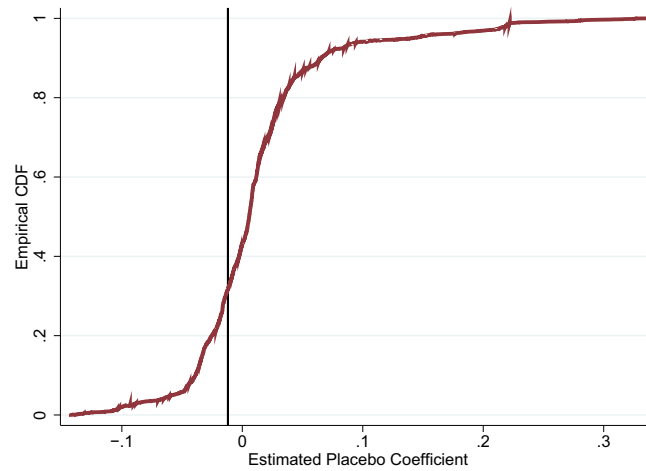
Fig. A8. Distribution of placebo coefficients. Notes: The figure displays the results of the nonparametric permutation test detailed in Section 4.3. The solid vertical line indicates the coefficient $\hat{\delta}$ from Eq. (16) estimated on daily entry data (t = day) from June 6, 2009 to June 5, 2011, with the last year defined as the post-reform period. The solid, red curve plots the distribution of the placebo coefficient from the equation estimated on the prereform periods only. To obtain these placebo coefficients, I begin with the period June 6, 2007 to June 5, 2009, defining the last year as the post-reform period, and move systematically backwards up to July 1, 1995 in steps of one week.
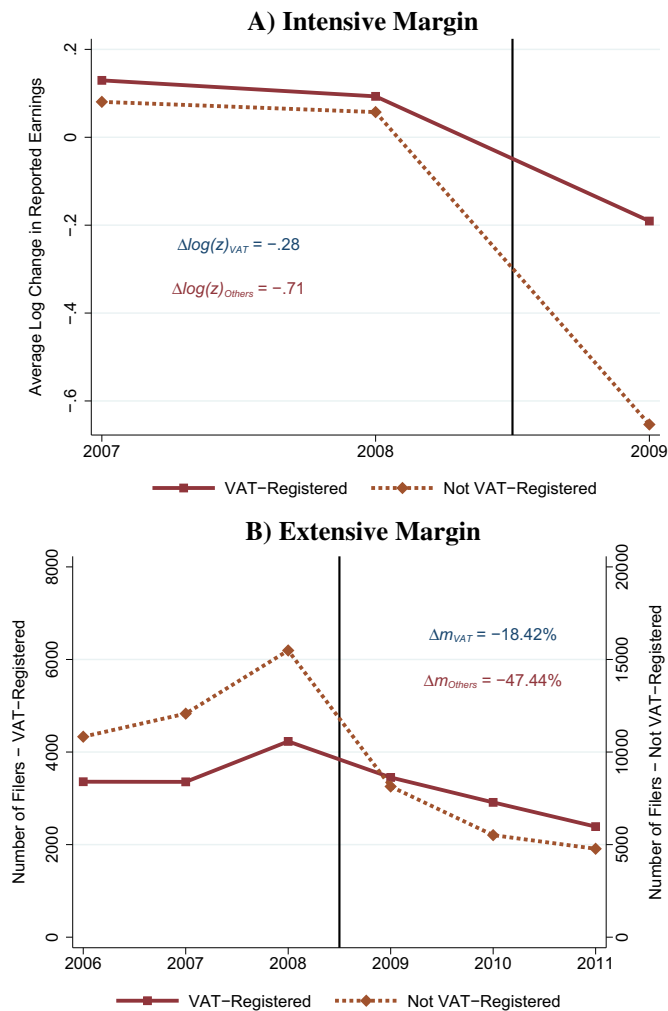
Fig. A9. Spillover effects on value-added tax base. Notes: The figure replicates the analysis in Figs. 4 and 5, stratifying the sample by VAT registration of a firm. For space considerations, curves related to the treatment group (partnerships) only are shown. Panel A displays earnings growth path of the two group of firms in 2006–09. To help the comparison, average log change in reported earnings between 2008 and 2009 has been shown in the figure separately for the two types of firms. Panel B illustrates the number of partnerships that report positive earnings in a given year t. Again, to help comparison, change in the number of firms from 2008 to 2009 as a percentage of the number of firms in 2008 are shown in the figure.
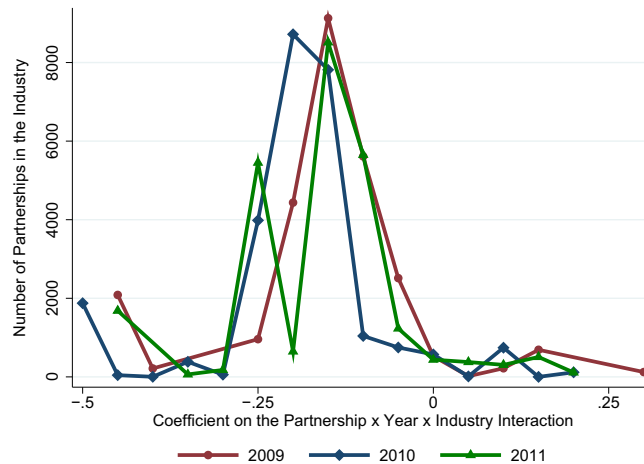
Fig. A10. Extensive margin response by industry. Notes: The figure explores heterogeneity in the extensive response across industries. I estimate the triple-difference version of Eq. (14), including industry and Partnership × Year × Industry dummies into the specification. The figure plots the distribution of the coefficients on the triple-interaction-dummies for the three post-reform years, showing that the response was fairly homogeneous with the exception of a few outliers. These outlier industries are Retail Sales; Manufacture of plastic, concrete, and plaster products; Repair of Machinery, Equipment, and Vehicles (coefficient < −0.35) and Real Estate Services, Accommodation Services, and Construction of Buildings (coefficient > 0).
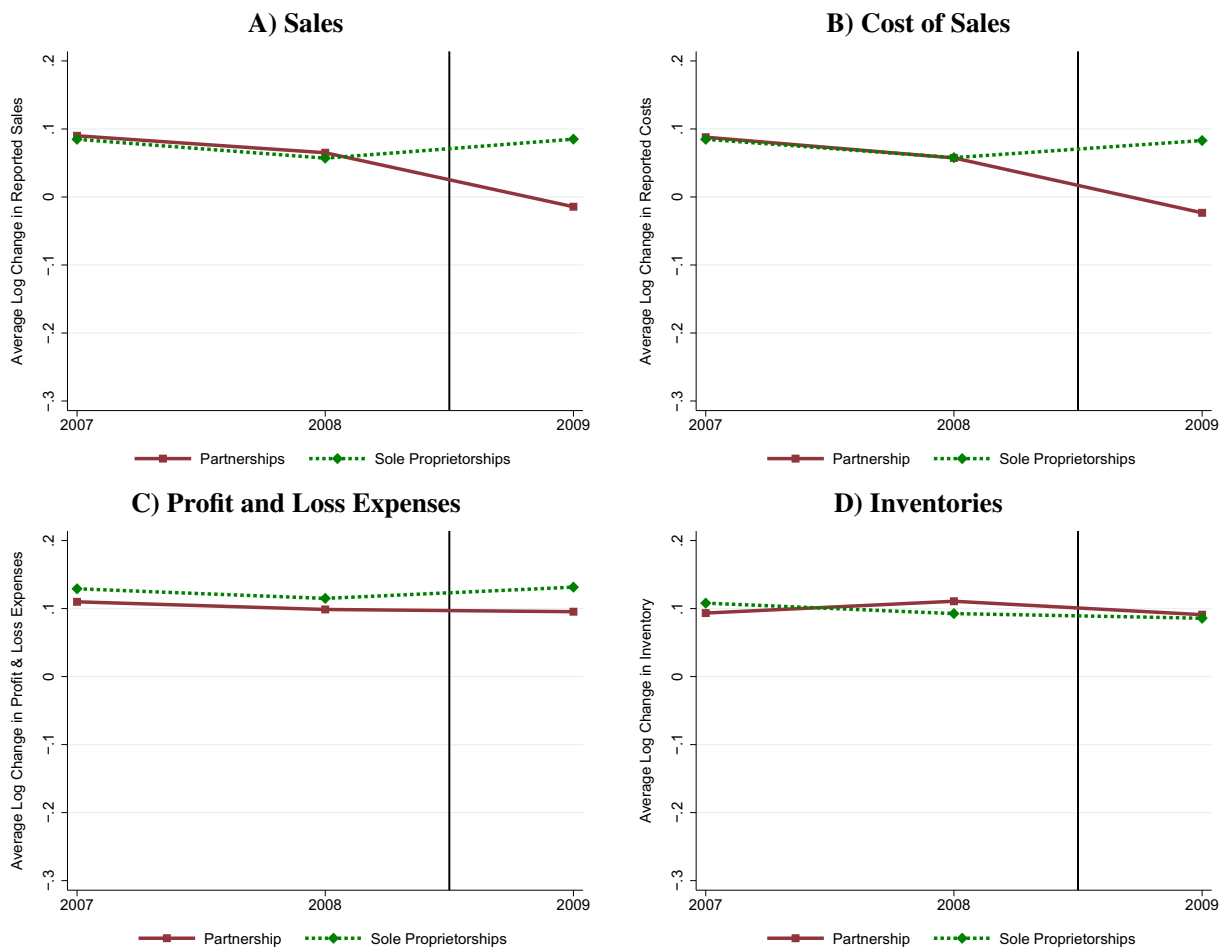
Fig. A11. Real vs. evasion response. Notes: The figure plots the evolution of individual line items reported on the tax return form between 2006 and 2009. Treatment group here are partnership firms, and the control group are sole proprietorships. The line items considered are the standard items in a profit and loss account. Specifically, Sales represents the money received in lieu of goods sold and/or services provided; Cost of Sales represents the direct cost of making those sales; Profit and Loss Expenses include wages, rents, utility payments, legal and administrative fee; and Inventories are the opening stock on day one of period *t*. Each marker on the curves denotes average, within-firm log change in the line item between the years *t* − 1 and *t* for the corresponding group of firms. The solid vertical line in each panel indicates the time from which the tax changes take effect.

Table A2
Intensive margin elasticities (2009) — balanced panel.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Sole proprietorships as control										
Elasticity	2.219	2.221	2.221	2.069	2.073	2.219	2.195	2.193	2.031	2.035
	(0.071)	(0.071)	(0.071)	(0.083)	(0.084)	(0.071)	(0.075)	(0.075)	(0.089)	(0.089)
Placebo	0.031	0.033	0.035	0.125	0.130	0.031	0.052	0.053	0.121	0.126
	(0.049)	(0.049)	(0.049)	(0.064)	(0.064)	(0.049)	(0.049)	(0.049)	(0.063)	(0.063)
Observations	538,560	538,560	524,559	104,421	104,412	538,560	538,560	524,559	104,421	104,412
B: Corporations as control										
Elasticity	2.347	2.291	2.337	2.104	2.017	2.019	1.967	2.010	1.784	1.717
	(0.352)	(0.360)	(0.353)	(0.351)	(0.358)	(0.231)	(0.242)	(0.228)	(0.238)	(0.255)
Placebo	−0.783	−0.847	−0.793	−0.747	−0.825	−0.089	−0.132	−0.095	−0.079	−0.139
	(0.369)	(0.382)	(0.363)	(0.377)	(0.428)	(0.184)	(0.194)	(0.182)	(0.191)	(0.210)
Observations	9603	9603	9594	6810	6801	9603	9603	9594	6810	6801
Controls										
Region Fixed Effects	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Tax Office Fixed Effects	No	No	Yes	No	Yes	No	No	Yes	No	Yes
Industry Fixed Effects	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Time Trend	Flexible	Flexible	Flexible	Flexible	Flexible	Linear	Linear	Linear	Linear	Linear

Notes: The table replicates the results in Table 2 on a balanced panel sample containing firms that report positive earnings in all periods included in the estimation 2006–09. Standard errors are in parenthesis, which are clustered at the firm level. The treatment group comprises partnership firms, and the results in Panels A and B are from using sole proprietorships and corporations as the control group. The estimates in column (1) are from the baseline specification; columns (2)–(5) add additional control variables. I do not observe the industry and tax office for all firms, owing to which the sample for the corresponding specifications is smaller than that for the others. Columns (6)–(10) replace year fixed effects in Eq. (12) with a linear time trend. Placebo results are from the corresponding specification estimated on the period 2006–08, with 2008 assumed as the post-reform period. The estimates are weighted by taxable income so that the elasticity corresponds to the parameter ε_p in Eq. (10).

Table A3
Intensive margin elasticities (2009) — robustness I.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Sole proprietorships as control								
Elasticity	2.233	2.219	2.219	2.193	2.189	2.129	2.153	2.236
	(0.077)	(0.071)	(0.079)	(0.075)	(0.112)	(0.117)	(0.079)	(0.092)
Placebo	0.025	0.031	0.036	0.051	0.036	0.051	0.125	0.131
	(0.044)	(0.049)	(0.044)	(0.049)	(0.044)	(0.049)	(0.051)	(0.068)
Observations	848,466	538,560	848,466	538,560	848,466	538,560	174,475	104,421
B: Corporations as control								
Elasticity	1.915	2.347	1.963	2.019	2.187	2.129	1.520	2.103
	(0.273)	(0.352)	(0.241)	(0.231)	(0.112)	(0.117)	(0.306)	(0.410)
Placebo	−0.222	−0.783	−0.179	−0.089	−0.179	−0.089	0.003	−0.938
	(0.447)	(0.369)	(0.202)	(0.184)	(0.202)	(0.184)	(0.464)	(0.446)
Observations	32,722	9603	32,722	9603	32,722	9603	21,338	6810
Specification								
Sample	All	Balanced panel	All	Balanced panel	All	Balanced panel	All	Balanced panel
Time Trend	Flexible	Flexible	Linear	Linear	Separate linear	Separate linear	Industry-specific	Industry-specific

Notes: The table presents intensive margin elasticity estimates from Eq. (12) estimated on the period 2006–09, permuting among the combinations of three alternative time trend and balanced-panel specifications. Standard errors are in parenthesis, which are clustered at the firm level. The treatment group comprises partnership firms, and the results in Panels A and B are from using sole proprietorships and corporations as the control group. Balanced-panel specifications include firms that report positive earnings in all periods included in the estimation 2006–09. Year fixed effects in Eq. (12) are replaced with a linear time trend in columns (3)–(4), separate linear time trends in columns (5)–(6), and industry-specific trends in columns (7)–(8). Industry-specific time trend specification includes a complete set of 2-digit industry, year, and industry ×year fixed effects, permitting firms in each industry their own earnings-growth trend. Placebo results are from the corresponding specification estimated on the period 2006–08, with 2008 assumed as the post-reform period. The estimates are weighted by taxable income so that the elasticity corresponds to the parameter ε_p in Eq. (10).

Table A4
Intensive margin elasticities (2009) — DFL reweighting.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Sole proprietorships as control										
Elasticity	2.079	2.085	2.075	2.098	2.093	2.090	2.095	2.084	2.111	2.100
	(0.106)	(0.109)	(0.107)	(0.106)	(0.107)	(0.108)	(0.111)	(0.109)	(0.108)	(0.109)
Placebo	0.050	0.050	0.052	0.050	0.054	0.026	0.028	0.031	0.025	0.034
	(0.069)	(0.069)	(0.069)	(0.068)	(0.068)	(0.067)	(0.067)	(0.067)	(0.066)	(0.066)
Observations	103,004	103,004	102,981	103,004	102,981	103,004	103,004	102,981	103,004	102,981
B: Corporations as control										
Elasticity	1.899	2.051	2.054	1.995	2.093	1.904	2.009	2.019	2.022	2.087
	(0.399)	(0.420)	(0.396)	(0.386)	(0.398)	(0.332)	(0.342)	(0.312)	(0.317)	(0.311)
Placebo	−0.263	−0.100	−0.007	−0.010	0.292	−0.135	0.005	0.057	−0.033	0.127
	(0.657)	(0.674)	(0.643)	(0.634)	(0.647)	(0.302)	(0.321)	(0.302)	(0.289)	(0.310)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Observations	12,997	12,997	12,954	12,997	12,954	12,997	12,997	12,954	12,997	12,954
Controls										
Region Fixed Effects	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Tax Office Fixed Effects	No	No	Yes	No	Yes	No	No	Yes	No	Yes
Industry Fixed Effects	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Time Trend	Flexible	Flexible	Flexible	Flexible	Flexible	Linear	Linear	Linear	Linear	Linear

Notes: The table replicates the results in Table 2 after reweighting the two control samples to match the treatment sample on size and industry dimensions using the DiNardo et al. (1996) method. I group partnerships into 250 (= 25 two-digit industries × 10 within-industry size deciles) bins according to the within-industry size-decile distribution of partnerships in 2008. Then within each firm type and year I adjust each bin's weight so that it carries the same relative weight as the 2008 partnership distribution. Standard errors are in parenthesis, which are clustered at the firm level. The treatment group comprises partnership firms, and the results in Panels A and B are from using sole proprietorships and corporations as the control group. The estimates in column (1) are from the baseline specification; columns (2)–(5) add additional control variables. I do not observe the industry and tax office for all firms, owing to which the sample for the corresponding specifications is smaller than that for the others. Columns (6)–(10) replace year fixed effects in Eq. (12) with a linear time trend. Placebo results are from the corresponding specification estimated on the period 2006–08, with 2008 assumed as the post-reform period. The estimates are weighted by taxable income so that the elasticity corresponds to the parameter ε_p in Eq. (10).

Table A5
Intensive margin elasticities (2009) — robustness II.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Sole proprietorships as control										
Elasticity	2.213	2.235	2.233	1.973	1.986	2.210	2.233	2.229	1.957	1.965
	(0.077)	(0.078)	(0.077)	(0.075)	(0.075)	(0.079)	(0.080)	(0.080)	(0.077)	(0.077)
Placebo	0.031	0.034	0.036	0.090	0.095	0.029	0.030	0.032	0.074	0.082
	(0.044)	(0.044)	(0.044)	(0.052)	(0.052)	(0.044)	(0.044)	(0.044)	(0.050)	(0.050)
Observations	889,500	889,500	851,920	187,218	187,191	889,500	889,500	851,920	187,218	187,191
B: Corporations as control										
Elasticity	1.915	2.112	2.169	1.664	1.893	1.963	2.125	2.240	1.744	1.974
	(0.273)	(0.280)	(0.256)	(0.264)	(0.255)	(0.241)	(0.247)	(0.210)	(0.221)	(0.203)
Placebo	−0.222	−0.212	0.051	0.020	0.212	−0.179	−0.120	0.003	−0.094	0.071
	(0.447)	(0.485)	(0.408)	(0.430)	(0.426)	(0.202)	(0.212)	(0.177)	(0.194)	(0.185)
Observations	32,722	32,722	32,640	21,338	21,272	32,722	32,722	32,640	21,338	21,272
Controls										
Region Fixed Effects	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Tax Office Fixed Effects	No	No	Yes	No	Yes	No	No	Yes	No	Yes
Industry Fixed Effects	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Time Trend	Flexible	Flexible	Flexible	Flexible	Flexible	Linear	Linear	Linear	Linear	Linear

Notes: While estimating the intensive margin elasticities reported in all tables other than this, from income shifting concerns I drop firm from the control group whose owners report income from a partnership in any of the six periods considered in this study 2006–11. This table replicates the analysis in Table 2 without dropping these firms to show that it is a careful precaution only, and that the results with and without these observations are indistinguishable. The treatment group comprises partnership firms, and the results in Panels A and B are from using sole proprietorships and corporations as the control group. The estimates in column (1) are from the baseline specification; columns (2)–(5) add additional control variables. I do not observe the industry and tax office for all firms, owing to which the sample for the corresponding specifications is smaller than that for the others. Columns (6)–(10) replace year fixed effects in Eq. (12) with a linear time trend. Placebo results are from the corresponding specification estimated on the period 2006–08, with 2008 assumed as the post-reform period. The estimates are weighted by taxable income so that the elasticity corresponds to the parameter ε_p in Eq. (10).

Table A6
Intensive margin elasticities (2009) — time series.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Elasticity	2.233	2.311	2.282	1.958	1.995	2.256	2.262	2.261	2.115	2.127
	(0.079)	(0.083)	(0.081)	(0.079)	(0.080)	(0.074)	(0.074)	(0.074)	(0.085)	(0.085)
Placebo	0.038	0.049	0.041	0.042	0.047	0.062	0.065	0.064	0.114	0.118
	(0.044)	(0.044)	(0.044)	(0.049)	(0.050)	(0.049)	(0.049)	(0.049)	(0.062)	(0.062)
Observations	29,489	29,489	29,473	18,135	18,135	9114	9114	9114	6324	6324
Controls										
Region Fixed Effects	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Tax Office Fixed Effects	No	No	Yes	No	Yes	No	No	Yes	No	Yes
Industry Fixed Effects	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Sample	All	All	All	All	All	Balanced panel	Balanced panel	Balanced panel	Balanced panel	Balanced panel

Notes: The table presents intensive margin elasticity estimates from the time-series analog of Eq. (12) estimated on the period 2006–09. Standard errors are in parenthesis, which are clustered at the firm level. The estimates in column (1) are from the baseline specification; columns (2)–(5) add additional control variables. Columns (6)–(10) replicate the results for a balanced-panel sample, which contains firms that report positive earnings in all four periods included in the estimation 2006–09. I do not observe the industry and tax office for all firms, owing to which the sample for the corresponding specifications is smaller than that for the others. Placebo results are from the corresponding specification estimated on the period 2006–08, with 2008 assumed as the post-reform period. The estimates are weighted by taxable income so that the elasticity corresponds to the parameter ε_p in Eq. (10).

Table A7

Intensive margin elasticities (2009–11) — time series.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Elasticity (2009)	2.233	2.267	2.248	1.951	1.957	1.852	1.859	1.856	1.785	1.791
	(0.079)	(0.082)	(0.080)	(0.079)	(0.079)	(0.102)	(0.102)	(0.102)	(0.112)	(0.112)
Elasticity (2010)	0.188	0.279	0.225	0.285	0.307	0.465	0.413	0.477	0.426	0.363
	(0.064)	(0.069)	(0.065)	(0.075)	(0.078)	(0.088)	(0.089)	(0.088)	(0.099)	(0.101)
Elasticity (2011)	0.155	0.293	0.200	0.301	0.357	0.650	0.548	0.665	0.701	0.609
	(0.063)	(0.070)	(0.064)	(0.076)	(0.086)	(0.098)	(0.097)	(0.098)	(0.114)	(0.115)
Observations	41,267	41,267	41,250	27,509	27,509	5850	5850	5850	4600	4600
Controls										
Region Fixed Effects	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Tax Office Fixed Effects	No	No	Yes	No	Yes	No	No	Yes	No	Yes
Industry Fixed Effects	No	No	No	Yes	Yes	Yes	No	No	Yes	Yes
Sample	All	All	All	All	All	Balanced panel	Balanced panel	Balanced panel	Balanced panel	Balanced panel

Notes: The table presents intensive margin elasticity estimates from the time-series analog of Eq. (12) estimated on the period 2006–11. Standard errors are in parenthesis, which are clustered at the firm level. The estimates in column (1) are from the baseline specification; columns (2)–(5) add additional control variables. Columns (6)–(10) replicate the results for a balanced-panel sample, which contains firms that report positive earnings in all six periods included in the estimation 2006–11. I do not observe the industry and tax office for all firms, owing to which the sample for the corresponding specifications is smaller than that for the others. The estimates are weighted by taxable income so that the elasticity corresponds to the parameter ε_p in Eq. (10).

Table A8

Extensive margin response.

	(1)	(2)	(3)	(4)	(5)
A: Sole proprietorships as control					
Partnership × 2009	−0.182	−0.167	−0.168	−0.173	−0.154
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Partnership × 2010	−0.227	−0.192	−0.204	−0.229	−0.196
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Partnership × 2011	−0.176	−0.140	−0.147	−0.203	−0.166
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Placebo	0.038	0.039	0.039	0.066	0.061
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Observations	3,037,310	3,037,310	2,864,532	954,840	954,822
B: Corporations as control					
Partnership × 2009	−0.220	−0.195	−0.207	−0.177	−0.150
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Partnership × 2010	−0.250	−0.209	−0.240	−0.205	−0.163
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Partnership × 2011	−0.298	−0.266	−0.283	−0.253	−0.224
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Placebo	0.047	0.038	0.042	0.041	0.027
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	283,575	283,575	283,103	223,616	223,616
Controls					
Region Fixed Effects	No	Yes	No	No	Yes
Tax Office Fixed Effects	No	No	Yes	No	Yes
Industry Fixed Effects	No	No	No	Yes	Yes
Prereform Mean	0.579	0.579	0.578	0.531	0.531

Notes: The table presents the results from Eq. (14) estimated on the period 2006–2011 using the linear probability model. Standard errors are in parenthesis, which are clustered at the firm level. The treatment group comprises partnership firms, and the results in Panels A and B are from using sole proprietorships and corporations as the control group. The sample includes all firms that file a return in period t and report earnings in the range $z_{it} \in [0\ 650K]$. Owing to the tax rule that all *registered* firms need to file a tax return a large number of firms with zero earning file return every year. The estimates in column (1) are from the baseline specification; columns (2)–(5) add additional control variables. I do not observe the industry and tax office for a number of firms, owing to which the sample in columns (3)–(5) is smaller than that in the first two columns. The placebo results are from the corresponding specification estimated on the same period 2006–11 with an additional interaction term Partnership × 2008 included in the regression. The rows titled Placebo show the coefficient and standard error on this additional interaction term. The last row reports the mean value of the outcome variable in the treatment group in the three prereform periods 2006–08.

Table A9
Extensive margin response — robustness.

	(1)	(2)	(3)	(4)	(5)	(6)
A: Sole proprietorships as control						
Partnership × 2009	−0.182	−0.117	−0.191	−0.095	−0.158	−0.105
	(0.003)	(0.005)	(0.003)	(0.005)	(0.003)	(0.006)
Partnership × 2010	−0.227	−0.144	−0.217	−0.127	−0.212	−0.134
	(0.003)	(0.006)	(0.003)	(0.006)	(0.004)	(0.007)
Partnership × 2011	−0.176	−0.104	−0.207	−0.144	−0.187	−0.131
	(0.003)	(0.006)	(0.003)	(0.006)	(0.004)	(0.007)
Placebo	0.038	0.008	0.077	0.041	0.063	0.017
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)
Observations	3,037,310	1,167,770	3,037,310	1,167,770	954,840	303,602
B: Corporations as control						
Partnership × 2009	−0.220	−0.158	−0.219	−0.137	−0.157	−0.113
	(0.004)	(0.009)	(0.003)	(0.006)	(0.005)	(0.010)
Partnership × 2010	−0.250	−0.189	−0.261	−0.189	−0.188	−0.136
	(0.005)	(0.011)	(0.004)	(0.008)	(0.005)	(0.011)
Partnership × 2011	−0.298	−0.252	−0.265	−0.227	−0.237	−0.197
	(0.005)	(0.011)	(0.004)	(0.009)	(0.006)	(0.012)
Placebo	0.047	−0.002	0.082	0.041	0.035	−0.013
	(0.004)	(0.008)	(0.002)	(0.003)	(0.004)	(0.008)
Observations	283,575	50,401	283,575	50,401	223,616	43,410
Sample	All	Balanced panel	All	Balanced panel	All	Balanced panel
Time Trend	Flexible	Flexible	Linear	Linear	Industry-specific	Industry-specific
Prereform Mean	0.579	0.513	0.579	0.513	0.531	0.491

Notes: The table presents the results from Eq. (14) estimated on the period 2006–2011 using the linear probability model, permuting over the combinations of time-trend and balanced-panel specifications. Standard errors are in parenthesis, which are clustered at the firm level. The treatment group comprises partnership firms, and the results in Panels A and B are from using sole proprietorships and corporations as the control group. Balanced-panel specifications include firms that file returns in all periods included in the estimation 2006–11. Year fixed effects in Eq. (14) are replaced with a linear time trend in columns (3)–(4), and industry-specific trends in columns (5)–(6). Industry-specific time trend specification includes a complete set of 2-digit industry, year, and industry ×year fixed effects, permitting firms in each industry their own earnings-growth trend. The placebo results are from the corresponding specification estimated on the same period 2006–11 with an additional interaction term Partnerships × 2008 included in the regression. The rows titled Placebo show the coefficient and standard error on this additional interaction term. The last row reports the mean value of the outcome variable in the treatment group in the three prereform periods 2006–08.

Table A10
Extensive margin response — probit.

	(1)	(2)	(3)	(4)	(5)
A: Sole proprietorships as control					
Partnership × 2009	−0.175	−0.138	−0.145	−0.167	−0.139
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Partnership × 2010	−0.204	−0.153	−0.169	−0.210	−0.173
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Partnership × 2011	−0.192	−0.144	−0.150	−0.217	−0.179
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Placebo	0.064	0.061	0.062	0.071	0.064
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	3,037,310	3,037,309	2,864,532	954,840	954,822
B: Corporations as control					
Partnership × 2009	−0.183	−0.163	−0.173	−0.150	−0.129
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Partnership × 2010	−0.217	−0.204	−0.211	−0.188	−0.172
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Partnership × 2011	−0.221	−0.199	−0.212	−0.196	−0.175
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Placebo	0.078	0.074	0.075	0.073	0.062
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	283,575	283,575	283,103	223,595	223,595
Controls					
Region Fixed Effects	No	Yes	No	No	Yes
Tax Office Fixed Effects	No	No	Yes	No	Yes
Industry Fixed Effects	No	No	No	Yes	Yes
Prereform Mean	0.579	0.579	0.578	0.531	0.531

Notes: The table replicates the analysis in Table A8, presenting the results from Eq. (14) estimated on the period 2006–2011 using the probit rather than the linear probability model. The reported coefficients are the marginal effects, calculated as the difference in probabilities of reporting positive earnings, one with the double-interaction term set equal to one and the other with the interaction term set to zero. Standard errors are in parenthesis, which are clustered at the firm level. The sample includes all firms that file a return in period t and report earnings in the range $z_{it} \in [0\ 650\ \text{K}]$. Owing to the tax rule that all *registered* firms need to file a tax return a large number of firms with zero earning file return every year. The estimates in column (1) are from the baseline specification; columns (2)–(5) add additional control variables. I do not observe the industry and tax office for a number of firms, owing to which the sample in columns (3)–(5) is smaller than that in the first two columns. The placebo results are from the corresponding specification estimated on the same period 2006–11 with an additional interaction term Partnership ×2008 included in the regression. The rows titled Placebo show the coefficient and standard error on this additional interaction term. The last row reports the mean value of the outcome variable in the treatment group in the three prereform periods 2006–08.

Table A11

Extensive margin response — sole proprietorships vs. corporations.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Corporations as control								
Sole Proprietorship × 2009	−0.022	−0.045	−0.006	−0.037	−0.004	−0.037	−0.001	0.000
	(0.003)	(0.007)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.000)
Sole Proprietorship × 2010	−0.108	−0.131	−0.056	−0.114	−0.053	−0.114	−0.058	−0.092
	(0.003)	(0.007)	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)	(0.008)
Placebo	−0.033	−0.010	−0.020	0.003	−0.020	0.003	−0.027	−0.013
	(0.003)	(0.006)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.006)
Observations	2,916,721	1,132,278	2,916,721	1,132,278	2,916,721	1,132,278	890,868	277,584
Specification								
Sample	All	Balanced panel	All	Balanced panel	All	Balanced panel	All	Balanced panel
Time Trend	Flexible	Flexible	Linear	Linear	Separate linear	Separate linear	Industry-specific	Industry-specific
Prereform Mean	0.754	0.784	0.754	0.784	0.754	0.784	0.609	0.640

Notes: The table explores if the sole proprietorship extensive margin outcomes are affected by the 2010 tax changes. I report the results from equations similar to Eq. (14) estimated on a sample containing sole proprietorships and corporations, using the linear probability model. The top two rows report the coefficients and standard errors on the two difference-in-differences terms, capturing how the sole proprietorship firms' probability to report positive earnings changed in 2010 and 2011 relative to corporations. The placebo result report the coefficient and standard error on the additional interaction term Sole Proprietorship × 2009, capturing any preexisting difference in outcomes across the two group of firms. Standard errors in parenthesis, which have been clustered at the firm level. Balanced-panel specifications include firms that file returns in all periods included in the estimation 2006–11. Year fixed effects in the baseline specification are replaced with a linear time trend in columns (3)–(4), separate linear time trends in columns (5)–(6), and industry-specific trends in columns (7)–(8). Industry-specific time trend specification includes a complete set of 2-digit industry, year, and industry × year fixed effects, permitting firms in each industry their own earnings-growth trend. The last row reports the mean value of the outcome variable for sole proprietorships in the four pre-2010.

Table A12

Income shifting to sole proprietorships.

	$1(z_{p,it} > 0)$		$1(z_{s,it} > 0)$		$1(z_{it} > 0)$	
	(1)	(2)	(3)	(4)	(5)	(6)
A: Sole proprietorships as control						
Partnership × 2009	−0.239	−0.245	0.137	0.131	−0.206	−0.212
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Partnership × 2010	−0.324	−0.311	0.223	0.236	−0.182	−0.169
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Partnership × 2011	−0.292	−0.319	0.306	0.278	−0.157	−0.184
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Placebo	0.090	0.129	−0.043	−0.005	0.039	0.078
	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Observations	2,991,995	2,991,995	2,991,995	2,991,995	2,991,995	2,991,995
B: Corporations as control						
Partnership × 2009	−0.282	−0.285	0.093	0.088	−0.246	−0.245
	(0.005)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
Partnership × 2010	−0.358	−0.372	0.187	0.171	−0.213	−0.220
	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)
Partnership × 2011	−0.411	−0.401	0.185	0.192	−0.274	−0.252
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Placebo	0.141	0.138	−0.028	−0.027	0.088	0.085
	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)
Observations	192,604	192,604	192,604	192,604	192,604	192,604
Time Trend	Flexible	Linear	Flexible	Linear	Flexible	Linear
Prereform Mean	0.701	0.701	0.224	0.224	0.859	0.859

Notes: The table explores income shifting to sole proprietorship business form, by presenting estimates from regressions similar to Eq. (15) using the linear probability model. The treatment group comprises partnership owners, defined as individuals who report earnings from a partnership in any of the three preform periods 2006–08. The results in Panels A and B are from using sole proprietors and corporations as the control group. The three outcome variables are: agent i reports positive partnership earnings (z_p) in period t (columns (1)–(2)); agent i reports positive sole proprietorship earnings (z_s) in period t (columns (3)–(4)); and agent i reports positive overall earnings $z = z_p + z_s$ in period t (columns (5)–(6)). For the two control groups the outcome variable is coded 1 if firm i reports positive taxable income in period t and zero if it reports zero taxable income. The placebo result report the coefficient and standard error on the additional interaction term Partnership × 2008, capturing any preexisting difference in outcomes across the treatment and control groups. The last row reports the mean value of the outcome variable in the treatment group in the three preform periods 2006–08. Standard errors are in parenthesis, which have been clustered at the firm level.

Table A13

Income shifting to sole proprietorships — BP.

	$1(z_{p,it} > 0)$		$1(z_{s,it} > 0)$		$1(z_{it} > 0)$	
	(1)	(2)	(3)	(4)	(5)	(6)
A: Sole proprietorships as control						
Partnership × 2009	−0.218	−0.196	0.110	0.131	−0.154	−0.132
	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Partnership × 2010	−0.278	−0.261	0.195	0.212	−0.119	−0.102
	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)
Partnership × 2011	−0.234	−0.275	0.269	0.228	−0.108	−0.148
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Placebo	0.060	0.094	−0.042	−0.010	−0.001	0.032
	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	1,171,375	1,171,375	1,171,375	1,171,375	1,171,375	1,171,375
B: Corporations as control						
Partnership × 2009	−0.240	−0.233	0.087	0.081	−0.177	−0.157
	(0.009)	(0.007)	(0.008)	(0.006)	(0.009)	(0.006)
Partnership × 2010	−0.328	−0.317	0.145	0.137	−0.169	−0.139
	(0.009)	(0.008)	(0.009)	(0.007)	(0.009)	(0.007)
Partnership × 2011	−0.370	−0.350	0.133	0.128	−0.244	−0.197
	(0.010)	(0.010)	(0.010)	(0.008)	(0.010)	(0.008)
Placebo	0.061	0.110	−0.041	−0.036	−0.000	0.044
	(0.009)	(0.006)	(0.008)	(0.004)	(0.008)	(0.004)
Observations	47,147	47,147	47,147	47,147	47,147	47,147
Time Trend	Flexible	Linear	Flexible	Linear	Flexible	Linear
Prereform Mean	0.612	0.612	0.319	0.319	0.815	0.815

Notes: The table replicates the results in Table A12 on a balanced panel of firms that file return in all periods included in the estimation 2006–11. The treatment group comprises partnership owners, defined as individuals who report earnings from a partnership in any of the three prereform periods 2006–08. The three outcome variables are: agent i reports positive partnership earnings (z_p) in period t (columns (1)–(2)); agent i reports positive sole proprietorship earnings (z_s) in period t (columns (3)–(4)); and agent i reports positive overall earnings $z = z_p + z_s$ in period t (columns (5)–(6)). For the two control groups the outcome variable is coded 1 if firm i reports positive taxable income in period t and zero if it reports zero taxable income. The placebo result report the coefficient and standard error on the additional interaction term Partnership ×2008, capturing any preexisting difference in outcomes across the treatment and control groups. The last row reports the mean value of the outcome variable in the treatment group in the three prereform periods 2006–08. Standard errors are in parenthesis, which have been clustered at the firm level.

Table A14

Income shifting to sole proprietorships — robustness.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Sole proprietorships as control								
Elasticity (2009)	−0.238	−0.190	−0.212	−0.211	−0.130	−0.133	−0.190	−0.177
	(0.007)	(0.011)	(0.006)	(0.010)	(0.010)	(0.017)	(0.013)	(0.020)
Elasticity (2010)	−31.246	−27.247	−34.370	−30.756	−24.639	−21.234	−24.912	−27.100
	(0.545)	(0.885)	(0.560)	(0.911)	(1.258)	(2.129)	(1.027)	(1.648)
Elasticity (2011)	−0.513	−0.452	−0.487	−0.398	−0.337	−0.244	−0.401	−0.410
	(0.007)	(0.011)	(0.007)	(0.011)	(0.020)	(0.034)	(0.013)	(0.020)
Observations	2,991,995	1,171,375	2,991,995	1,171,375	2,991,995	1,171,375	867,567	285,457
B: Corporations as control								
Elasticity (2009)	−0.161	−0.151	−0.154	−0.141	−0.130	−0.133	−0.153	−0.116
	(0.009)	(0.027)	(0.007)	(0.014)	(0.009)	(0.018)	(0.013)	(0.034)
Elasticity (2010)	−32.717	−25.896	−28.156	−22.443	−24.639	−21.234	−30.994	−23.102
	(0.944)	(2.766)	(0.718)	(1.588)	(1.154)	(2.258)	(1.345)	(3.456)
Elasticity (2011)	−0.378	−0.273	−0.396	−0.264	−0.337	−0.244	−0.368	−0.265
	(0.011)	(0.032)	(0.010)	(0.024)	(0.018)	(0.036)	(0.016)	(0.040)
Observations	192,604	47,147	192,604	47,147	192,604	47,147	110,372	21,767
Specification								
Sample	All	Balanced panel	All	Balanced panel	All	Balanced panel	All	Balanced panel
Time Trend	Flexible	Flexible	Linear	Linear	Separate linear	Separate linear	Industry-specific	Industry-specific

Notes: The table establishes the robustness the income shifting elasticities in Table 5 by reporting results from additional specifications. Standard errors are in parenthesis, which are clustered at the firm level. The treatment group comprises partnership owners, defined as individuals who report earnings from a partnership in any of the three prereform periods 2006–08. The results in Panels A and B are from using sole proprietors and corporations as the control group. Balanced-panel specifications include agents that report positive earnings in all periods included in the estimation 2006–11. Year fixed effects in the baseline specification are replaced with a linear time trend in columns (3)–(4), separate linear time trends in columns (5)–(6), and industry-specific trends in columns (7)–(8). Industry-specific time trend specification includes a complete set of 2-digit industry, year, and industry ×year fixed effects, permitting agents in each industry their own earnings-growth trend.

Table A15
Income shifting to corporations.

	(1)	(2)	(3)	(4)	(5)	(6)
A: Sole proprietorships as control						
Partnership × 2009	0.001	0.001	−0.000	−0.002	−0.004	−0.007
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Partnership × 2010	0.004	0.004	0.003	0.003	−0.002	−0.004
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)
Partnership × 2011	0.004	0.007	0.004	0.006	−0.002	−0.003
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.004)
Placebo	0.000	−0.000	0.001	0.003	0.001	0.003
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Observations	2,991,995	867,551	2,991,995	867,551	2,991,995	867,551
B: Corporations as control						
Partnership × 2009	0.001	0.001	0.000	−0.000	−0.004	−0.006
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Partnership × 2010	0.005	0.006	0.004	0.005	−0.002	−0.004
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)
Partnership × 2011	0.006	0.008	0.006	0.009	−0.002	−0.003
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.004)
Placebo	0.004	0.005	0.002	0.003	0.001	0.002
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Observations	192,595	110,363	192,595	110,363	192,595	110,363
Controls						
Region Fixed Effects	No	Yes	No	Yes	No	Yes
Tax Office Fixed Effects	No	Yes	No	Yes	No	Yes
Industry Fixed Effects	No	Yes	No	Yes	No	Yes
Time Trend	Flexible	Flexible	Linear	Linear	Separate linear	Separate linear
Prereform Mean	0.021	0.027	0.021	0.027	0.021	0.027

Notes: The table explores income shifting to the corporate business form. The estimates are from the difference-in-differences regression Eq. (15) with an indicator showing if *i* reports positive dividend income in period *t* as the outcome variable. The treatment group comprises partnership owners, defined as individuals who report earnings from a partnership in any of the three prereform periods 2006–08. The results in Panels A and B are from using sole proprietors and corporations as the control group. The placebo result report the coefficient and standard error on the additional interaction term Partnership × 2008, capturing any preexisting difference in outcomes across the treatment and control groups. The last row reports the mean value of the outcome variable in the treatment group in the three prereform periods 2006–08. Standard errors are in parenthesis, which have been clustered at the firm level.

Table A16
Income shifting to corporations — BP.

	(1)	(2)	(3)	(4)	(5)	(6)
A: Sole proprietorships as control						
Partnership × 2009	−0.001	−0.003	−0.002	−0.004	−0.005	−0.005
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.004)
Partnership × 2010	−0.000	0.001	0.000	0.001	−0.004	−0.001
	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.005)
Partnership × 2011	0.003	0.004	0.002	0.004	−0.003	0.002
	(0.001)	(0.003)	(0.001)	(0.003)	(0.003)	(0.006)
Placebo	0.001	0.001	0.002	0.002	0.002	0.003
	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)
Observations	1,171,375	285,457	1,171,375	285,457	1,171,375	285,457
B: Corporations as control						
Partnership × 2009	−0.002	−0.002	−0.003	−0.003	−0.005	−0.005
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.004)
Partnership × 2010	0.001	0.002	−0.001	0.002	−0.004	−0.002
	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.005)
Partnership × 2011	0.003	0.006	0.001	0.005	−0.003	0.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.006)
Placebo	0.002	0.003	0.002	0.003	0.002	0.003
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Observations	47,153	21,773	47,153	21,773	47,153	21,773
Controls						
Region Fixed Effects	No	Yes	No	Yes	No	Yes
Tax Office Fixed Effects	No	Yes	No	Yes	No	Yes
Industry Fixed Effects	No	Yes	No	Yes	No	Yes
Time Trend	Flexible	Flexible	Linear	Linear	Separate linear	Separate linear
Prereform Mean	0.028	0.031	0.028	0.031	0.028	0.031

Notes: The table replicates the results in Table A15 on a balanced panel sample containing agents that report positive taxable earnings in all periods included in the estimation 2006–2011. The estimates are from the difference-in-differences regression Eq. (15) with an indicator showing if *i* reports positive dividend income in period *t* as the outcome variable. The treatment group comprises partnership owners, defined as individuals who report earnings from a partnership in any of the three prereform periods 2006–08. The results in Panels A and B are from using sole proprietors and corporations as the control group. The placebo result report the coefficient and standard error on the additional interaction term Partnership × 2008, capturing any preexisting difference in outcomes across the treatment and control groups. The last row reports the mean value of the outcome variable in the treatment group in the three prereform periods 2006–08. Standard errors are in parenthesis, which have been clustered at the firm level.

Table A17
Heterogeneity in intensive margin response — firm characteristics.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Partnership × 2009	2.233	2.573	2.709	2.247	2.984	2.439	2.608	3.538
	(0.077)	(0.243)	(0.114)	(0.082)	(0.099)	(0.082)	(0.095)	(0.292)
Partnership × 2009								
× Electronic Return Filer		−0.565						0.055
		(0.250)						(0.284)
× Firm Size			−1.670					−0.730
			(0.157)					(0.178)
× Firm Age				−0.642				−0.432
				(0.184)				(0.231)
× Tax Withholding					−2.260			−1.906
					(0.134)			(0.154)
× Withholding Agent						−1.634		−0.195
						(0.170)		(0.232)
× VAT-Registered							−1.498	−0.927
							(0.137)	(0.172)
Observations	848,466	848,466	620,735	811,064	848,466	848,466	848,466	613,116

Notes: The table explores heterogeneity in intensive margin elasticities across firms with different characteristics. I report the results from triple-difference version of Eq. (12), including the firm characteristic and triple-interaction (Partnership × Year × Firm Characteristic) dummies into the regression. The estimates in column (1) report the income-weighted average elasticity; the estimates in the subsequent columns break down the elasticity by firm-characteristic indicated in each row. Details of the firm characteristics variables are given in Appendix A.1. Standard errors are in parenthesis, which are clustered at the firm level.

Table A18
Heterogeneity in intensive margin response — income groups.

Taxable Income ≤	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sole proprietorships as control										
250,000	3.134	3.167	3.155	2.846	2.865	3.106	3.140	3.129	2.821	2.839
	(0.117)	(0.118)	(0.117)	(0.113)	(0.113)	(0.117)	(0.119)	(0.118)	(0.113)	(0.114)
	[795,997]	[795,997]	[759,265]	[146,081]	[146,077]	[795,997]	[795,997]	[759,265]	[146,081]	[146,073]
350,000	2.829	2.856	2.846	2.585	2.597	2.808	2.838	2.826	2.558	2.566
	(0.097)	(0.098)	(0.097)	(0.092)	(0.092)	(0.098)	(0.099)	(0.098)	(0.092)	(0.093)
	[828,288]	[828,288]	[791,125]	[161,555]	[161,551]	[828,288]	[828,288]	[791,125]	[161,555]	[161,542]
450,000	2.542	2.567	2.558	2.311	2.323	2.526	2.553	2.542	2.286	2.293
	(0.083)	(0.084)	(0.083)	(0.079)	(0.079)	(0.084)	(0.085)	(0.085)	(0.080)	(0.080)
	[840,398]	[840,398]	[803,086]	[168,869]	[168,864]	[840,398]	[840,398]	[803,086]	[168,869]	[168,852]
550,000	2.338	2.360	2.356	2.116	2.128	2.331	2.354	2.348	2.096	2.105
	(0.076)	(0.077)	(0.076)	(0.075)	(0.075)	(0.078)	(0.079)	(0.078)	(0.076)	(0.077)
	[846,310]	[846,310]	[808,945]	[172,930]	[172,925]	[846,310]	[846,310]	[808,945]	[172,930]	[172,909]
650,000	2.233	2.253	2.251	1.999	2.009	2.219	2.241	2.238	1.973	1.981
	(0.077)	(0.078)	(0.077)	(0.074)	(0.075)	(0.079)	(0.080)	(0.080)	(0.077)	(0.077)
	[848,466]	[848,466]	[811,075]	[174,475]	[174,470]	[848,466]	[848,466]	[811,075]	[174,475]	[174,450]
Controls										
Region Fixed Effects	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Tax Office Fixed Effects	No	No	Yes	No	Yes	No	No	Yes	No	Yes
Industry Fixed Effects	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Time Trend	Flexible	Flexible	Flexible	Flexible	Flexible	Linear	Linear	Linear	Linear	Linear

Notes: The table explores heterogeneity in intensive margin response across high- vs. low-income firms. I report the results from Eq. (12), estimated on the period 2006–09. Treatment and control groups are partnerships and sole proprietorships respectively. Each row of the table reports results from the regression, restricting the sample to firms that have base period income up to the limit $\bar{z}$ indicated in the row *i.e.* $z_{it} \in (0 \ \bar{z}]$. Standard errors are in parenthesis, which have been clustered at the firm level. Number of observations in each regression are given in square brackets.

Table A19
Heterogeneity in extensive margin response — firm characteristics.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Partnership × 2009	−0.182	−0.385	−0.341	−0.202	−0.331	−0.207	−0.267	−0.494
	(0.003)	(0.006)	(0.005)	(0.003)	(0.004)	(0.003)	(0.003)	(0.011)
× Electronic Return Filer		0.277						0.088
		(0.006)						(0.012)
× Firm Size			0.232					0.153
			(0.010)					(0.011)
× Firm Age				0.095				0.061
				(0.006)				(0.011)
× Tax Withholding					0.191			0.115
					(0.008)			(0.010)

× Withholding Agent						0.254 (0.009)		0.087 (0.014)
× VAT-Registered							0.240 (0.005)	0.126 (0.010)
Observations	2,047,399	2,047,399	1,050,675	1,931,287	1,519,360	2,047,399	2,047,399	1,024,080

Notes: The table explores heterogeneity in extensive margin response across firms with different characteristics. I report the results from the following triple-difference version of Eq. (14) estimated on the period 2006–09

$$1(z_{it} > 0) = \alpha + \beta_0 \ Partnership_i + \beta_1 \ Xtc_i + \beta_2 \ Partnership \times 2009_{it} + \beta_3 \ Partnership \times 2009 \times Xtc_{it} + \lambda_t + u_{it},$$

where Xtc_i is the firm-characteristic variable. The estimates in column (1) capture the average, tax-driven reduction in the propensity to report positive earnings by partnership firms. The estimates in the subsequent columns break down the response by firm-characteristic indicated in each row: top rows of the columns report $\hat{\beta}_2$ and the bottom rows $\hat{\beta}_3$ from the above regressions. Details of the firm characteristics variables are given in Appendix A.1. Standard errors are in parenthesis, which are clustered at the firm level.

Table A20
Taxable income responses (partnerships vs. sole proprietorships).

Notch point	Earnings response			Structural elasticity		
(1)	Partnerships (2)	Sole proprietorships (3)	Difference (4)	Partnerships (5)	Sole proprietorships (6)	Difference (7)
200 K	15,500 (3283)	18,000 (5141)	−2500 (6099)	0.229 (0.102)	0.308 (0.207)	−0.079 (0.231)
300 K	24,000 (4468)	29,000 (5941)	−5000 (7434)	0.080 (0.036)	0.121 (0.053)	−0.041 (0.064)
400 K	24,500 (4075)	35,500 (7688)	−11,000 (8702)	0.041 (0.016)	0.095 (0.044)	−0.054 (0.047)
500 K	32,500 (7665)	34,500 (5894)	−2000 (9669)	0.045 (0.027)	0.052 (0.020)	−0.007 (0.034)
600 K	22,500 (5956)	30,000 (5342)	−7500 (8000)	0.008 (0.010)	0.028 (0.009)	−0.020 (0.013)

Notes: This table illustrates how partnerships and sole proprietorships compare in terms of their tax behavior. The table partially recreates Table II in Kleven and Waseem (2013) , and estimates earnings response and taxable income elasticity separately for sole proprietorships and partnerships, exploiting their bunching responses to the notches in the baseline tax system. Standard errors are in parentheses. The results show that though earnings responses and elasticities are relatively smaller for partnerships, the differences are not significant either statistically or economically.

Table A21
Industry distribution of firms.

Industry label	Industry description
(1)	(2)
1	Personal service activities including washing and dry-cleaning, hairdressing and other beauty treatment, funeral services
2	Non-specialized wholesale trade
3	Other manufacturing not elsewhere classified
4	Retail sale in non-specialized stores
5	Spinning, weaving, and finishing of textile products
6	Manufacture of knitted, crocheted, and other fabrics
7	Activities of business and employers membership
8	Retail sale of clothing, footwear and leather articles, pharmaceutical, and cosmetic goods in specialized stores
9	Motor repair services
10	Wholesale of construction materials, hardware, metals, and metal ores
11	Manufacture of prepared meals, chocolate, and sugar confectionary
12	Wholesale of machinery, equipment, and supplies
13	Growing of cereals (except rice), leguminous crops, and oil seeds
14	Wholesale of textiles, clothing, footwear, and other household goods
15	Construction of buildings
16	Retail sale of electrical household appliances, furniture, lighting equipment in specialized stores
17	Business support service activities including packaging
18	Sale of motor vehicles
19	Real estate activities with own or leased property
20	Wholesale of food, beverages, and tobacco
21	Manufacture of gas; distribution of gaseous fuels through mains
22	Manufacture of grain mill and starch products
23	Manufacture of sports goods
24	Manufacture of medical and dental instruments and supplies
25	Retail sale in specialized stores

Notes: This table presents the detailed description of the 25 industries shown in the two top panels of Fig. A1. Column (1) corresponds to the industry label shown along the x-axis of the plot. Column (2) provides the detailed description of the industry.

References

Andreoni, J., Erard, B., Feinstein, J., 1998. Tax complicance. J. Econ. Lit. 36, 818–860.

Bachas, P., Soto, M., 2017. Not(ch) your Average Tax System: Corporate Taxation Under Weak Enforcement. Working Paper.

Best, M.C., Brockmeyer, A., Kleven, H.J., Spinnewijn, J., Waseem, M., 2015. Production versus revenue efficiency with limited tax capacity: theory and evidence from Pakistan. J. Polit. Econ. 123 (6), 1311–1355.

Bruhn, M., McKenzie, D., 2014. Entry regulation and the formalization of microenterprises in developing countries. World Bank Res. Obs. 29 (2), 186–201.

Carrillo, P., Pomeranz, D., Singhal, M., 2017. Dodging the taxman: firm misreporting and limits to tax enforcement. Am. Econ. J. Appl. Econ. 9 (2), 144–164.

Chetty, R., 2009a. Is the taxable income elasticity sufficient to calculate deadweight loss? The implications of evasion and avoidance. Am. Econ. J. Econ. Pol. 1 (2), 31–52.

Chetty, R., 2009b. Sufficient statistics for welfare analysis: a bridge between structural and reduced-form methods. Annu. Rev. Econ. 1 (1), 451–488.

Cooper, M., McClelland, J., Pearce, J., Prisinzano, R., Sullivan, J., Yagan, D., Zidar, O., Zwick, E., 2015. Business in the United States: Who Owns It and How Much Tax Do They Pay? Working Paper 21651. National Bureau of Economic Research October.

DiNardo, J., Fortin, N.M., Lemieux, T., 1996. Labor market institutions and the distribution of wages, 1973–1992: a semiparametric approach. Econometrica 64 (5), 1001–1044.

Feldstein, M., 1999. Tax avoidance and the deadweight loss of the income tax. Rev. Econ. Stat. 81 (4), 674–680.

Fisman, R., Wei, S.-J., et al., 2004. Tax rates and tax evasion: evidence from "missing imports" in China. J. Polit. Econ. 112 (2), 471.

Fuest, C., Riedel, N., 2009. Tax Evasion, Tax Avoidance and Tax Expenditures in Developing Countries: A Review of the Literature. Report prepared for the UK Department for International Development (DFID).

Goolsbee, A., 2004. The impact of the corporate income tax: evidence from state organizational form data. J. Public Econ. 88, 2283–2299.

Gordon, R., Li, W., 2009. Tax structures in developing countries: many puzzles and a possible explanation. J. Public Econ. 93 (7-8), 855–866.

Gordon, R., Slemrod, J., 2000. Are 'real' responses to taxes simply income shifting between corporate and personal tax bases? In: Slemrod, J. (Ed.), Does atlas shrug? The economics consequences of taxing the rich.

Gordon, R.H., MacKie-Mason, J.K., 1997. How much do taxes discourage incorporation? J. Financ. 52 (2), 477–505.

Kleven, H.J., Knudsen, M.B., Kreiner, C.T., Pedersen, S., Saez, E., 2011, 05. Unwilling or unable to cheat? Evidence from a tax audit experiment in Denmark. Econometrica 79 (3), 651–692.

Kleven, H.J., Waseem, M., 2013. Using notches to uncover optimization frictions and structural elasticities: theory and evidence from Pakistan. Q. J. Econ. 128, 669–723.

Kleven, H.J., Kreiner, C.T., Saez, E., 2016. Why can modern governments tax so much? an agency model of firms as fiscal intermediaries. Economica 83 (330), 219–246.

Kopczuk, W., 2012. The Polish Business "Flat" Tax and its Effect on Reported Incomes: A Pareto Improving Tax Reform? Columbia University Working Paper.

Marion, J., Muehlegger, E., 2008. Measuring illegal activity and the effects of regulatory innovation: tax evasion and the dyeing of untaxed diesel. J. Polit. Econ. 116 (4), 633–666.

Nevo, A., 2000. A practitioner's guide to estimation of random-coefficients logit models of demand. J. Econ. Manag. Strateg. 9 (4), 513–548.

Saez, E., Slemrod, J., Giertz, S., 2012. The elasticity of taxable income with respect to marginal tax rates: a critical review. J. Econ. Lit. 50, 3–50.

Slemrod, J., Kopczuk, W., 2002. The optimal elasticity of taxable income. J. Public Econ. 84 (1), 91–112.

Slemrod, J., Yitzhaki, S., 2002. Tax avoidance, evasion, and administration. In: Auerbach, A.J., Feldstein, M. (Eds.), Handbook of Public Economics. vol. 3.

Waseem, M., 2017. Using the Movement of Exemption Cutoff to Estimate Tax Evasion: Evidence from Pakistan. University of Manchester Working Paper.